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BENIER'S HOT AIR ENGINE.

THE greatest drawback in hot air engines is that the materials used for lubrication are destroyed by the heat of the air—being, in fact, decomposed at a temperature of over 250°, and not lubricating well at a lower one. It is to such a circumstance that is especially to be attributed the want of success of such motors of this kind as have hitherto been constructed.

The Messrs. Benier, in their study of the motor which we illustrate herewith, have partially undertaken the task of finding a means of overcoming this difficulty without interfering with the operation or diminishing the performance of the engine, which, besides, must occupy a relatively small space.

Referring to the engravings, we shall, without further prefatory remarks, proceed to describe the engine's mode of operating.

At every revolution, the pump, *d*, sucks in a certain quantity of air while the crank is describing the arc, M N A. On another hand, when the crank is passing over the arc, A B, the slide valve closes the aperture, *m*, as shown in plan view in Fig. 4, and the piston of the pump compresses the air introduced into its cylinder.

At the point, B, of the revolution, the motive piston, *b*, has reached the end of its stroke, the eduction valve, *s*, is closed, and the slide valve, *t*, sets up a communication between the pump and motive cylinders. The compressed air enters the last named cylinder through two conduits. One of these, *n*, leads a portion of the air under the grate, *i*, upon making its exit from which it passes through the fire box. The hot gases derived from combustion fill the interior of the cylinder and act upon the motive cylinder during its entire ascending stroke. The other portion of the air enters the cylinder through the conduit, *k*, and fills the annular space comprised between the motive piston and cylinder. It will be seen, in fact, that the motive piston, at its lower part, from *o* to *p*, has a diameter one millimeter less than that of its rubbing part, and that fresh air constantly fills the annular space. Consequently, the hot gases, laden with ashes, that fill the cylinder cannot come into contact with the rubbing surfaces.

The pump piston continues to force fresh air into the motive cylinder up to the end of its stroke. At this moment the crank is at M, and the slide valve shuts off communication between the motive cylinder and the pump. It thus opens a communication with the latter and the atmosphere, so that the latter may be sucked in. From M to N the piston continues its stroke under the action of the expansion of the gases contained in the cylinder. At N, as the piston is at the end of its upward stroke, the eduction valve, *s*, is raised, and the hot gases escape while the piston is descending.

The governor, situated within the column that carries the axle of the working beam, regulates the velocity by causing more or less air to enter the fire box, according as the motive power is to be greater or less. As may be seen, it actuates the valve, *r*, whose opening to the full extent corresponds to the maximum of power that the motor is capable of developing. In measure as it closes, more and more air passes through the conduit, *k*, and less through *n*. Consequently, the temperature of the gases that fill the cylinder becomes lower and lower, and the pressure, like the power developed, diminishes, as does also the fuel burned.

FIG. 2.

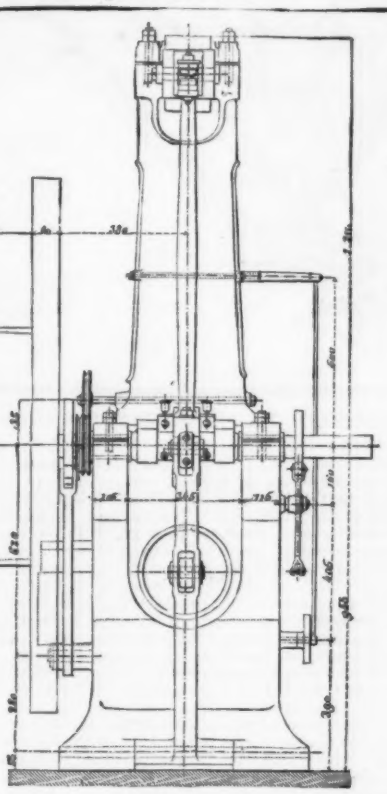
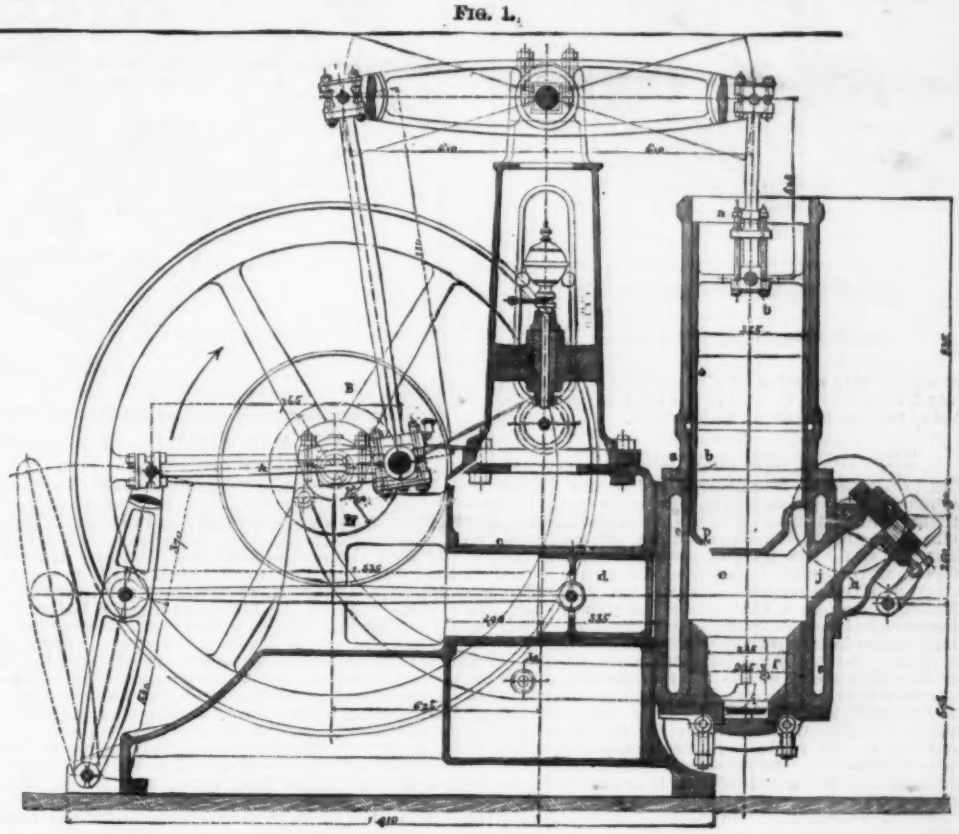


FIG. 1.



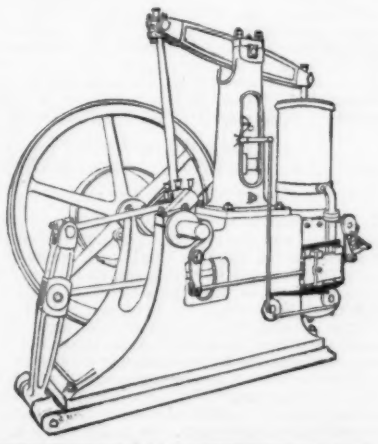


FIG. 5.

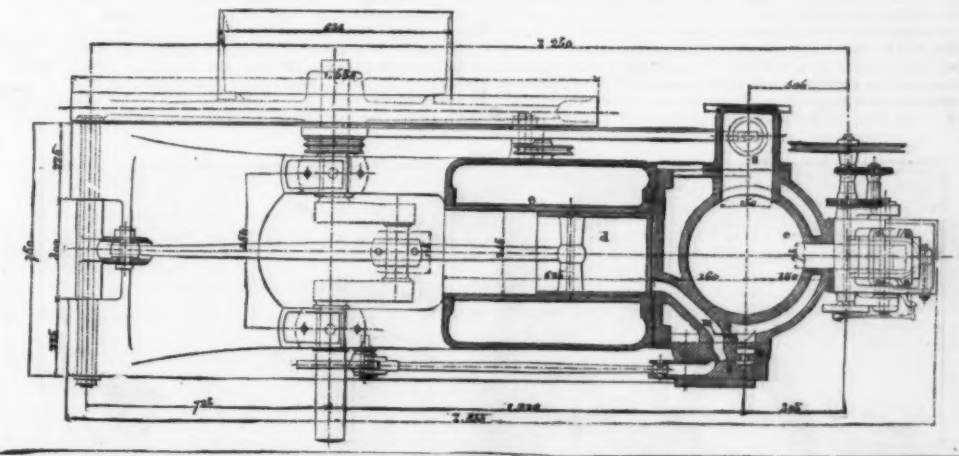


FIG. 4.

FIG. 1.—Vertical Section. FIG. 2.—End View. FIG. 4.—Horizontal Section along the Axis of the Pump. FIG. 5.—Perspective View of the Motor.

BENIER'S HOT AIR ENGINE.

The feeding of the furnace, which is within the cylinder, is effected through the valve, *g*, which makes about ten travels per minute. A boy or a young girl puts the pieces of coke one by one in the interior of this valve's cavity. The fuel falls into the conduit, *j*, and from there passes to the furnace, which is isolated from the cylinder by a lining of refractory clay.

The cleaning of the grate is effected through the door, *u*, which is removed before every starting up. Finally, during the running, the motive cylinder is cooled by a current of water that circulates all around its jacket, as in gas motors.

The machine is still so new that we are unable to give the exact figures of its performance. The Messrs. Benier guarantee a mean consumption of 3¼ pounds of coke per horse and per hour.

Description of Figures.—*a*, upper motive cylinder; *b*, motive piston; *c*, pump chamber; *d*, piston of the pump; *e*, lower motive cylinder; *f*, fire box; *g*, coke valve; *h*, coke box; *i*, grate; *j*, coke chute; *k*, upper air conduit; *m*, air conduit of the pump; *n*, lower air conduit of the cylinder; *o p*, part of the piston surrounded by air; *r*, valve of air conduit; *s*, eduction valve; *t*, slide valve; *u*, door of the fire box; A B M N, travel of the crank.—*Revue Industrielle.*

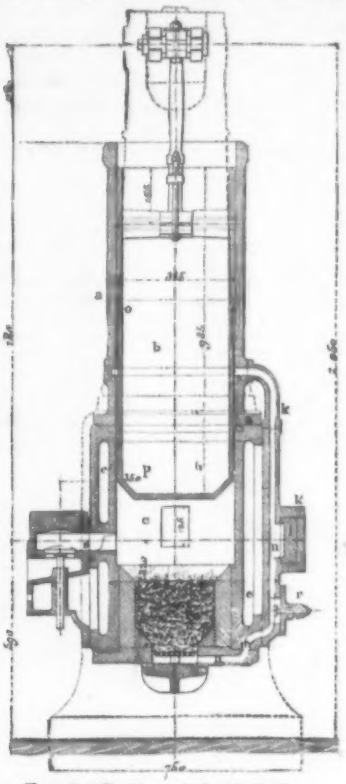
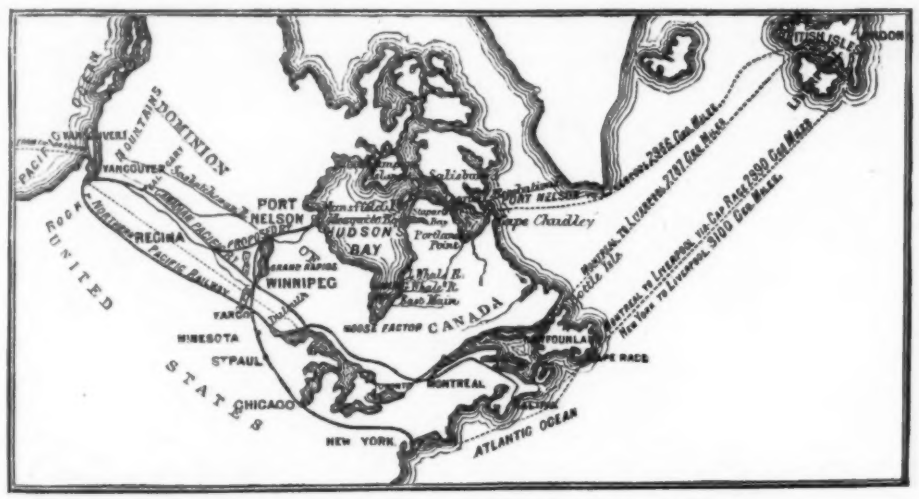


FIG. 3.—Section of the Cylinder.

THE HUDSON'S BAY RAILWAY.

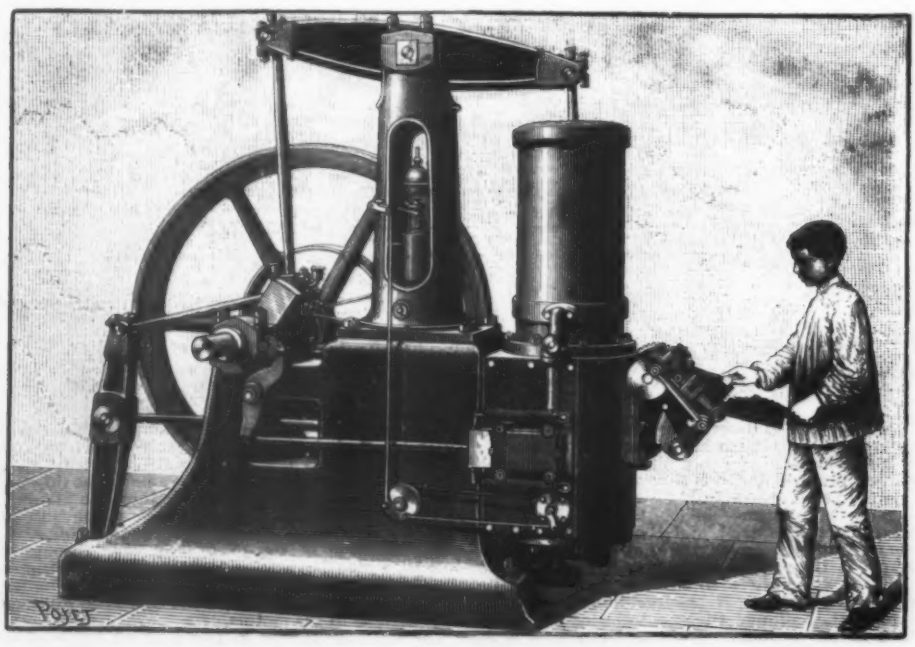
THE construction of railways within the United Kingdom has now reached a stage at which many railway engineers have to submit to intervals of inaction, and, as the natural and healthy result of this condition, to look abroad for new fields of activity; and it is fortunate that the spirit maintained between the old country and the colonies opens a wide field. Any undertaking, moreover, that affects the safety, speed, and economy of the transport of our food supplies has an importance in that respect which raises it from the rank of other colonial developments having only a local interest. Northwest Canada is pre-eminently one of the colonies to which we may look for a large supply of grain and meat, and there can be little doubt of the immediate beneficial reaction on London of an improvement in the trade route from Manitoba. The supply of breadstuffs and meat from that immense field of produce need be limited only by the capabilities of the transport arrangements. The energy displayed by the Dominion Government in fostering schemes for the improvement of internal communication is most striking and praiseworthy. The Canadian Pacific Railway is not only a monument of engineering skill and intrepidty in grappling with and overcoming natural obstacles of the most formidable character, but it is a substantial proof of what the Dominion Government is prepared to countenance and assist with the view of developing the magnificent territory over which it presides. Strategical motives, or motives of economy of distance, have, however, traced the Canadian Pacific Railway near the southern boundary of the Dominion, in effect on a nearly straight line from east to west, and north of it lie great granaries and pasture lands yet to be brought into touch with the old country. The railway in all its length from Montreal to Vancouver is like an elongated artery in the human body, and the veins to feed it from the north are still to be formed. Already a number of the necessary lines into the northern provinces are projected or in course of construction, and there can be no doubt that in the course of time we shall hear of their successful completion, with the result of an ever-growing development of the home trade of the colony.

Among the lines authorized by the Canadian Parliament, and now in course of construction in Northwest Canada, there is one known as the Winnipeg and Hudson's Bay Railway, which has already attracted some attention in this country, and is likely to assume considerable importance if its promoters are successful in their object of developing and improving the old Hudson's Bay trade route to Great Britain. This route has been successfully used by the Hudson's Bay Company for over two hundred and fifty years, and was—until recently—the only means of communication with their numerous trading posts on the prairies; and if the number of vessels sent out by the company to Hudson's Bay is less now than it formerly was, it is because since the establishment of railways to the south of Manitoba it costs them less to export by this route a great portion of the goods that they formerly dispatched by way of York Factory *via* Hudson's Bay. By the authorized line of railway above named it is proposed to connect Regina, its western terminus, and Winnipeg, its eastern—both on the Canadian Pacific Railway—with Port Nelson. The projectors claim for their route, which is shown on the accompanying map, enormous savings in distance by railway, while the distance by sea is practically the same as by other routes. The savings in railway transit by the Hudson's Bay route to Liverpool are given as follows: From Winnipeg over Montreal route, 775 miles; from Winnipeg over New York route, 1129 miles; from Winnipeg over Halifax route, 1618 miles; from Regina over Montreal route, 1081 miles; from Regina over New York route, 1435 miles; from Regina over Halifax route, 1924 miles. As to the navigation, the projectors are supported by the most recent reports of evidence collected on the subject by the Dominion Government from their own agents, stationed at various points on the Straits and Bay. They say on this authority that navigation for ocean steamships will be open in the Straits for at least four months, and frequently for six months of the year; that the harbor selected near the mouth of the Nelson River is never closed by ice, and that the river itself has a tidal navigation of twenty-five miles.



MAP SHOWING PROPOSED HUDSON'S BAY RAILWAY.

As an advantage of detail, referring to the nature of the trade expected, it is pointed out that in the summer season the new route will be cooler, and that this peculiarity, added to the saving in distance, will tend to secure for it a large share of the meat and grain traffic from the United States as well as from Canada, because both of these classes of exports are benefited by a certain degree of cold. The line of railway—which has a total length of about 950 miles—is intended to follow the line of the Hudson Bay Company's trade route from Port Nelson to Lake Winnipeg, and by its western coast to reach the city of Winnipeg—the capital of Manitoba—and the famous Red River Valley. The western branch, separating at Grand Rapids, will serve Regina, the capital of the northwest territory and the center of the fertile belt. A considerable traffic is also anticipated by reason of the facilities which the railway will afford for the development of the fisheries of Hudson's Bay, which are known to be extensive. The whole line, for the purpose of construction, is divided into three sections: From Regina to Grand Rapids, 300 miles; from Winnipeg to Grand Rapids, 250 miles; and from Grand Rapids to Port Nelson, 400 miles. No particular engineering difficulties are anticipated, and only a small portion of the line traverses a rocky district, the remainder being located in prairie land or easy country. The sections throughout have been laid out with the view of economizing works. Some large timber trestle bridges of the Howe truss type will, however, be required to bridge the gorges in which the rivers run. The railway throughout, both in works and permanent way, will be built to the same excellent standard as that of the Canadian Pacific Railway; and it is satisfactory to learn that the works in connection with the section immediately north of Winnipeg are now in progress and are being vigorously prosecuted, a shipment of 4400 tons of steel rails from England having been already made, while about forty miles of the line have been graded. Provision will be made along the route and at Port Nelson for the storage and rapid shipment of large quantities of grain, and for this purpose grain elevators constructed on the model of those already in use on the Canadian Pacific Railway and in the United States will be erected. The Provincial Government of Manitoba, as an evidence of the keen interest with which the undertaking is regarded in the Northwest, has given a guarantee to assist in the execution of the works, and the Dominion Government has reserved a magnificent area of 8,400,000 acres from the Crown lands adjoining the railway as a subsidy or free gift. The contractors for the construction of the line have had considerable local experience, having been engaged upon the works of the Canadian Pacific and other railways in Canada. The consulting engineers of the company are Sir F. J. Bramwell, F.R.S., and Mr. W. Shelford.—*The Engineer.*



BENIER'S HOT AIR ENGINE.

ON THE DESIGN OF SLIDE VALVES.

By Prof. R. H. SMITH.

IT is not intended here to say anything of the merits or demerits of the various kinds of slide valves. These differ greatly as to the frictional work spent in driving them, as to their steam tightness, as to their wearing qualities, and as to their more or less rapid opening and closing of the ports. The simple geometrical question alone of the relation between steam distribution and valve dimensions is to be dealt with, the valve being supposed driven by an ordinary eccentric.

The diagram shown in the engraving is almost identical with one constructed by myself in 1870 or 1871. The elementary principle of the device was suggested to me by a German draughtsman. He had a small brass disk about 2 in. in diameter, on which was described a circle divided into equal parts, and a series of parallel, equidistant straight lines, perpendicular to a scribed diameter, which was taken as the piston stroke and the valve stroke. At the center was pivoted a small arbor, on which were mounted two dial pointers taken to represent crank and eccentric. These could be clamped together at any angle, and the two could be rotated together by means of a small milled head. The instrument was a very rough one, incapable of accurate calculation; but its chief fault lay in the fact that no account was taken of the effect of the obliquity of the connecting rod.

The convenience of the instrument consisted in its obviating the necessity of drawing any circles on paper and of plotting off any angles with the dividers in working out slide-valve problems. It was an easy step to perceive that the same result could be obtained by a carefully drawn diagram on paper or cardboard, with various scales marked on it, and that in preparing such a diagram allowance could be made for the obliquity of the connecting rod by projecting the equal divisions of the piston stroke on the crank pin circle by means

RULES TO FIND PROPER VALVE DIMENSIONS REQUIRED FOR GIVEN STEAM DISTRIBUTION.

1. *Given Pre-admission to find Angle of Lead:*

Find the desired point of pre-admission on the proper piston-stroke scale, and the point opposite this on the scale of angle-degrees. The angle between this point and the end of the stroke is the Angle of Lead. The sum of the Angle of Lead and of Angle of Lap is the angular advance.

2. *Given Pre-admission and Cut-off to find Outside Lap:*

Find Angle of Lead by Rule 1, and set it forward from point of cut-off found on proper piston-stroke scale. Half the remaining angle between the point so got and the end of the stroke is the Angle of Lap.

3. *Given Pre-admission, Cut-off, and Release to find Inside Lap:*

Find angular advance by Rules 1 and 2. Set the angular advance back from end of stroke. The angular difference between the point so got and the desired point of release as found on the proper piston-stroke scale is the Angle of Inside Lap, or of *clearance if release is further back than point first got.*

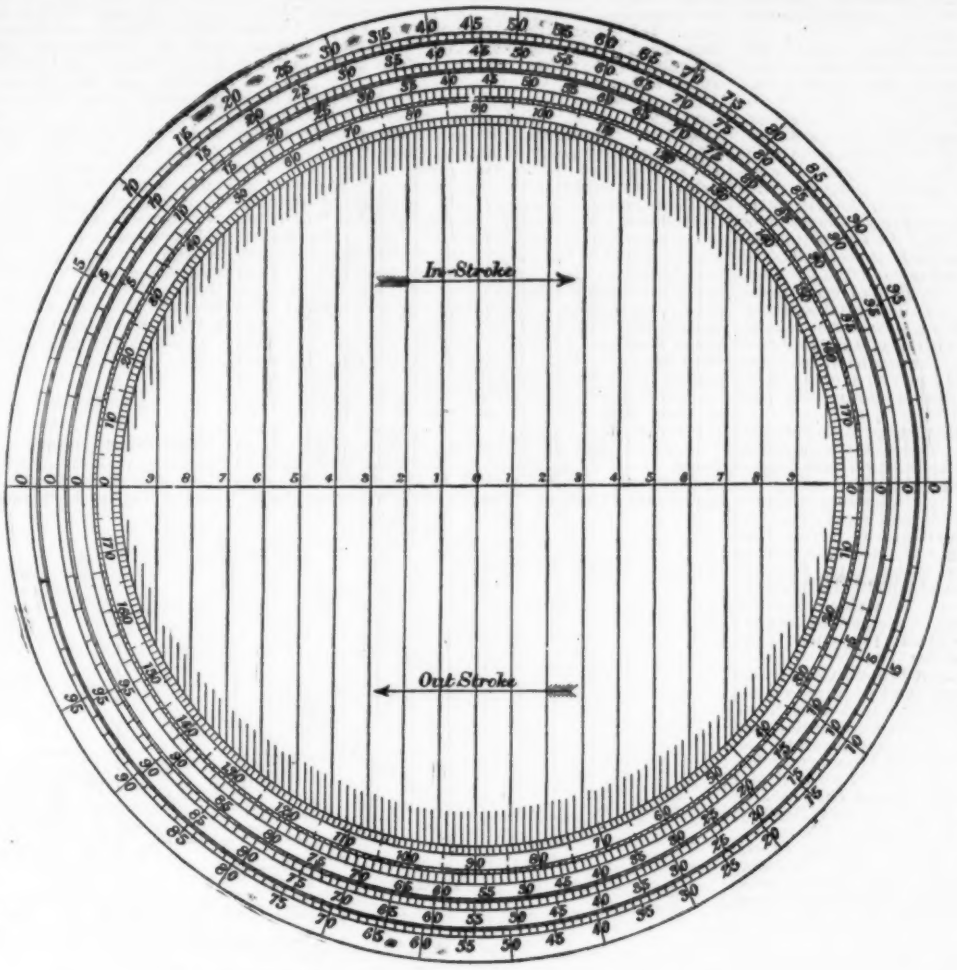
4. *Given Pre-admission, Cut-off, and Compression to find Inside Lap:*

Find angular advance by Rules 1 and 2. Set the angular advance back from the beginning of the stroke. The angular difference between the point so got and the desired point of compression as found on the proper piston-stroke scale is the Angle of Inside Lap or of *clearance if the point of compression is further forward than the point first got.*

5. *Given Cut-off and Angular Advance to find Outside Lap:*

From point of cut-off as found on proper piston-stroke scale, set forward the angular advance. The angle from point so got to end of stroke is the Angle of Outside Lap.

SLIDE VALVE DIAGRAM.



Scale on Diameter.—Valve travel; each half-travel divided into 50 equal parts. (These divisions being projected on 1st circle.)

Scale on 1st Circle.—Equal degrees of Angle; each semicircle divided into 180 degrees.

Scale on 2d Circle.—Piston Stroke divided into 100 equal parts; Connecting-rod = 2 Strokes. (Showing positions of Crank Pin for simultaneous positions of Piston.)

Scale on 3d Circle.—Piston Stroke divided into 100 equal parts; Connecting-rod = 2½ Strokes.

Scale on 4th Circle.—Piston Stroke divided into 100 equal parts; Connecting-rod = 3 Strokes.

RULES TO FIND STEAM DISTRIBUTION DUE TO GIVEN VALVE DIMENSIONS.

1. *Given Lap and Angular Advance to find Pre-admission:*

Subtract from Angle of Lap and Lead, *i. e.*, the angular advance, the Angle of Lap. Set the difference of these angles back from the beginning of the stroke. The point so got is the point of Pre-admission.

2. *Given Lap and Angular Advance to find Cut-off:*

Add the angular advance to the Angle of Lap. Set the sum of these angles back from the end of the stroke. The point so obtained is the point of Cut-off.

3. *Given Inside Lap and Angular Advance to find Release or Exhaust:*

Subtract from angular advance the Angle of Inside Lap. Set the difference of these angles back from the end of the stroke. The point so got is the point of Release.

4. *Given Inside Lap and Angular Advance to find Compression:*

Add the angular Advance to the Angle of Inside Lap. Set the sum of these angles back from the beginning of the stroke. The point so got is the point of Compression.

DEFINITIONS.

The "Angular Advance" is the angle between crank and eccentric, less 90 deg.

The "Angle of Lap" is the angle whose sine is the outside lap divided by the half-travel.

of circular arcs of connecting rod radius, instead of by perpendicular straight lines. In the diagram attached to this article this is done for three standard ratios of connecting rod length to crank throw. The connecting rod lengths chosen are 2, 2½, and 3 strokes. The stroke is divided into 100 equal parts, and the corresponding positions of the crank pin are shown on the three outer circular scales for the above three connecting rod lengths. The actual connecting rod length in steam engines is never so much different from one or other of these as to make any appreciable difference in the slide valve calculation.

The inner circle is taken as 5 in. in diameter, and represents to different scales the paths of both crank pin and eccentric center. The horizontal diameter represents to the same two scales the strokes of piston and of valve. It is divided into 100 parts, only every fifth division being indicated by a vertical cross line. Considering it as the valve stroke, the corresponding positions of the eccentric center are projected on the circle by vertical straight lines, because the obliquity of the eccentric rod may be taken as practically *nil.* In slide valve design, all dimensions have to be considered in ratio to the half travel of the valve. Therefore, the half travel is conveniently divided into ten parts, each of these being subdivided into five. The one-fiftieths of the half travel are large, and easily divisible by eye, so that there is no difficulty in reading off the ratios to 1 per cent. of half travel.

In using the diagram, careful distinction must be made between the out and in strokes. The upper semicircle must be used in dealing with the in stroke, the lower with the out stroke. The diagram must be so used, whatever be the actual direction of rotation of the engine. The diagram can be used for the solution of two classes of converse problems—first, given the valve and port dimensions and the setting of the valve, to find the resulting steam distribution; secondly, given the desired steam distribution and the ratio of connecting rod length to stroke, to find the required valve dimensions and eccentric setting. In conjunction with the diagram, two sets of rules are given—first, four for the first class of problems, and second, five for the converse problems. These rules are so easily proved to be correct by a little careful consideration that it is hardly worth while to give demonstrations of their truth here. It will be sufficient to make one or two explanations as to the phraseology used and as to the general mode of procedure.

In the rules, the phrase "set off" such an angle is constantly used. This setting off is not to be performed with dividers. By help of the scale of angle degrees all the angles used are obtained in degrees, and the simplest mental addition and subtraction of degrees are alone needed for the plotting off of the angles from any of the points mentioned in the rules. If the mental arithmetical faculty be not developed in the operator, he can make his additions and subtractions on paper, but he should not substitute the use of the dividers for this arithmetic, because the prickings of the divider points would rapidly destroy the diagram for all accurate purposes. The "angle of lead" is to be understood as the angle at which the crank stands before the dead point when steam admission takes place. The "angle of lap" is the angle beyond 90 deg. at which the eccentric stands when steam admission takes place. The "angular advance," is the sum of these two angles.

It is to be distinctly understood that the diagram enables one to calculate only angles and ratios of the valve dimensions. Evidently, any standard diagram could not give directly absolute dimensions. The port breadth has to be found from the required area of port. Call it b. Say that a ratio, q, has been calculated by help of the diagram for outside lap divided by half travel. Then from this we find the half travel $= \frac{b}{1-q}$

and therefore the outside lap $= b\frac{q}{1-q}$

If we wish to make the cut-off the same on out and in stroke, we find two different values of q for the two strokes, *i. e.*, for the two outside laps. That obtained for the out stroke is always the larger of the two if the two angles of lead be taken the same. For example, if the connecting rod were two strokes in length, and the cut off $\frac{7}{10}$ were desired for both out and in strokes, and an angle of lead of 10 deg. were wished, then for in stroke we would have "angle of lap" $= \frac{1}{2}(180-129-10)$ deg.) $= 25$ deg., and consequently $q = 0{\cdot}42$. For the out stroke we find "angle of lap" $= \frac{1}{2}(180-106\frac{1}{2}-10) = 31\frac{1}{4}$ deg., and therefore $q = 0{\cdot}53$. If now b were taken the same for both ports, the half travel as calculated from the first figure, $0{\cdot}42$, $\left(\frac{b}{1-0{\cdot}42} = \frac{b}{0{\cdot}58}\right)$ would be less than that from the second, $0{\cdot}53$, $\left(\frac{b}{1-0{\cdot}53} = \frac{b}{0{\cdot}47}\right)$.

Now the two half travels cannot be unequal. We must therefore have either unequal cut-offs, or unequal port openings for the two ends of the cylinder. It will generally be preferable to have unequal port openings. We must then calculate the half travel from one of the two ratios, q, obtained, and from the q obtained for the other end of the valve calculate the port opening and lap from this half travel. Thus, suppose that in the above example it has been reckoned that ¾ in. steam port opening was required. If we calculate half travel from the $0{\cdot}42$ for in stroke, we would have half travel $\frac{0{\cdot}75}{0{\cdot}58} = 1{\cdot}29''$, and lap $= 1{\cdot}29 - 0{\cdot}75 = 0{\cdot}54$. Then for out stroke we would have lap $= 1{\cdot}29 \times 0{\cdot}53 = 0{\cdot}68$, and therefore port opening $1{\cdot}29 - 0{\cdot}68 = 0{\cdot}61$. But this port opening is less than has been settled as necessary. We must therefore proceed in the other order, and calculate the half travel from the $0{\cdot}53$ obtained for the out stroke; thus half travel $= \frac{0{\cdot}75}{1-0{\cdot}53} = 1{\cdot}60$, and therefore lap $= 1{\cdot}60 - 0{\cdot}75 = 0{\cdot}85$. For in stroke we then obtain lap $= 1{\cdot}60 \times 0{\cdot}42 = 0{\cdot}67$, and therefore steam port opening $= 1{\cdot}60 - 0{\cdot}67 = 0{\cdot}93$. This is in excess of the required opening, but so far as simple steam admission is concerned, this excess is no evil, whereas the deficiency obtained by the previous dimensions is a decided evil. The larger valve travel of course involves more waste of frictional work in driving the valves, and in some cases this evil may be considered to overbalance the evil of deficient opening of the steam port at one end. But this cannot be considered the standard case, and therefore we should generally follow the rule to design the half travel by help of the ratio, q, obtained for the cut-off on out stroke. We then from this half travel calculate the lap for in stroke, and we will find that the steam port opening for in stroke is in excess of what is absolutely required.—*The Engineer.*

ON THE REMARKABLE EFFECTS OF ADDING SACCHARINE MATTER TO MORTARS.

By SAMUEL CROMPTON.

A LETTER from Mr. Thomson Hankey "On a New Use of Sugar"—*London Times*, October 13—has given rise to wide discussion and inquiry. At a time when the price of sugar is so low, and, as I shall show, when the use of a very small quantity of it, or of treacle, adds largely to the strength of mortar, and makes Portland cement itself set with great rapidity, it seems to me that I may do a service to engineers by laying before them the scientific grounds on which I was led to experiment on the subject, and the remarkable results which have been obtained.

The practical importance of this addition of saccharine matter to mortar I will state briefly to begin with, and will give a few illustrations:

I mixed in a small jar some Portland cement and brown sugar, adding water and stirring. I took out a little of the cement for an experiment, and when I tried an hour after to take out more, I found that the remainder had already set.

My neighbor, Mr. Rowland, weighed carefully Portland cement and sand into four small jars. To two he added different sugars, to the third treacle, but to the fourth no saccharine matter. On the day following the cement had set—we do not know how much earlier, for it was not examined—in all the jars with saccharine matter. Mr. Holden, Jr., the foreman of a builder, examined all of them on the Monday following the Friday on which they had been mixed. On pressing the cement to which the treacle had been added, he said, "I might press the bottom of the jar out before I can make impression on this." He then put his finger into the jar in which there was no saccharine matter, and stirred up the cement, which had not set at all, and which did not set till a day or two after. It may be objected that it

might not be an advantage that it should set so quickly. This objection will be answered by and by, as I proceed, by showing that it is highly probable that the strength of Portland cement will be greatly increased by this addition of saccharine matter.

Mr. Thomson Hankey had experiments made, first by his own brickmaker and secondly by a house builder. Both reported that the addition of sugar made a common lime equal to Portland cement.

The bearing of these facts will be plain to every engineer, but I cannot forbear mentioning here a startling incident. The Ecclesiastical Commissioners built for the late Bishop Fraser, of Manchester, on his coming into the diocese, a lodge like that at Lambeth, with a lofty archway set in Portland cement. A clerk of the works appointed by the architect of the commissioners superintended it. After a due time the scaffolding was removed. One day—perhaps that day—the Bishop walked through and had just got beyond danger when the whole of the archway fell. If he had been under it, he would have been killed, and his grand career as a bishop would have been cut short at the outset. Your readers, as I proceed with my statement, will judge for themselves whether the addition of a few shillings' worth of treacle would not have made all secure, and have saved the expense of doing this work over again.

I have to explain how it is that sugar, or rather saccharine matter of any kind, produces this remarkable effect on lime. Here I ought to mention that I am a retired physician, and that the idea of putting the matter to the proof arose in the following manner: In medicine we have two kinds of lime water; one, the common lime water that can be got by mixing lime and water. It is to be particularly noted that, add as much lime as you like, it is impossible to get water to dissolve more than half a grain of lime in one ounce, or about two small tablespoonfuls, of water. But by adding two parts of white sugar to one part of lime, we obtain a solution containing about 14½ times more lime in the same quantity of water.

Here it is to be observed—and it is a most important point—that there are hot limes, such as Buxton lime, which, if the sugar be incautiously mixed with them, will *burn* the sugar, make it a deep brown color, and convert it into other chemical forms, and possibly, and I think probably, will destroy its value in mortar. The way to use sugar with such limes is to dissolve it first in the water. I dwell particularly upon this, because a gentleman referred to me by Mr. Thomson Hankey, in writing to thank me for the information I had given him, casually observed that his cement had turned nearly black by the addition of the sugar. Probably many other experimenters with sugar and hot limes have had the same result, and are in the belief that all is right. Our strong medical saccharated lime water looks like water.

Ten or fifteen years ago I had been experimenting with lime and sugar, but not in reference to mortar, and I spoke about that time to my friend, the late E. W. Binney, F.R.S., about this property of sugar. He said that it was very curious, and that it was new to him; and he told me this anecdote, that in his grandfather's time an Italian architect came down to Worksop to erect a building for a nobleman, and insisted on being supplied with malt to make his mortar with; the malt was supplied and used. Many years afterward, this building had to be taken down; but, said Binney, "they could not pull it down, do what they would, and they had to use gunpowder." I said it would be the saccharine matter in the malt that produced this result. He agreed with me.

A few months ago I was at Peterborough, and went to see the progress of the restoration of the cathedral, where I made the acquaintance of Mr. Irvine, Mr. Pearson's clerk of the works. Mr Irvine was for more than a quarter of a century with Sir Gilbert Scott, and possesses a greater knowledge of architecture, and antiquity bearing on English architecture, than any one I ever met with. One day I said to him that I had been to Fotheringay. He replied that he had seen every other church than that in the neighborhood of Peterborough. I asked him to go with me on his Saturday's half-holiday, as my visit to the church had been a hurried one, and I wished to make some further inquiries. Besides, I was glad to have the companionship of one who was so thorough a master of the subject. The chancel of this fine church, built before the nave, and so late as 1410, has entirely perished; and it had been so badly built even that in the time of Queen Elizabeth it had fallen in and was then in ruins. The chancel and tower exist, but the tower is unsafe, and if the church be not soon restored, this grand historical monument may suffer or be destroyed. As Mr. Irvine and I walked to the railway station, I asked him whether he was aware of the chemical fact that the addition of sugar to water makes it take up about sixteen times more lime than water by itself does—I might have said a little more than fourteen times. He replied that he did not know this fact, and that he had never heard of it, and that he did not believe that it had been so used in mortars. I then told him what Mr. Binney had told me regarding the building erected by the Italian architect near Worksop. He said that he had been clerk of the works in the restoration of several cathedrals, in the books of which he had met with old entries of payments, "For beer for the masons," and that he had found one entry where it was written, "For beer to mix with the mortar." I said that that would be for the saccharine matter in it; and I added that a few years ago I had seen in a newspaper that the vintage in Spain had been so abundant that the people had not casks enough, and were using the wine to mix with their mortar. It flashed across my mind that this traditional use of saccharine matter was probably the explanation of the exceptional hardness of the old Roman mortar, and had been handed down from generation to generation, and had at length been forgotten, in England at any rate. A few days afterward I was pondering as I walked along the street in Peterborough on this matter, when suddenly I said to myself, "Why not try the experiment?" I went into a grocer's shop and bought a pound of exceedingly finely powdered loaf sugar and some beeswax—of the wax I will speak some other time. I took the sugar to the hut in the cathedral yard, where I found the foreman of the contractor and Mr. Irvine. Laughing, I said, "I have come to teach you to suck eggs." After explaining to the intelligent foreman my views, he and Mr. Irvine kindly agreed to try the experiment. Some powdered lias lime and some of the sugar were being mixed together in an iron basin. Water was added, and Mr. Irvine began to stir them with a trowel. No sooner had he done so than he exclaimed, "Look, look! It is beginning to set already." I said, "Is not that usual?" He replied, "No; something very uncommon." The mortar was poured out on the end of a beam, where it set.

Some more was then made much thinner, and a little sand added to it. With this, which was about the consistency of cream, two largish fragments of the broken stone tracery of an old window were joined, and so were two bricks, two pieces of glass, and two slates. It would be about five o'clock in the evening. As I was going to leave Peterborough about noon on the following day, I called at the cathedral about ten in the morning to see the results. Mr. Irvine said it was too early to judge. He felt at the stone tracery very tenderly. Holding the upper fragment, he then tilted the tracery sideways, and as the stones held together, he then took hold of the upper fragment with both hands and lifted the whole stone without the lower fragment falling off. In like manner, in lifting both bricks the lower brick did not fall off. The slate and the glass seemed also set. So that the experiments seemed to confirm remarkably the view I had formed, on theoretical and chemical grounds chiefly, that saccharine matter added to mortar would be of great value, and that an important discovery had been made. I wrote to my brother-in-law, Mr. Guildford Molesworth, engineer-in-chief to the State railways in India, and author of "The Engineer's Pocket Book," telling him what we had done. From him I received a letter dated Simla, August 28, 1886, giving me the following interesting particulars: "With regard to your addition of sugar to mortar, it is a practice that has been in use in the Madras Presidency from time immemorial." The following is an extract from the Roorha (?) "Treatise on Civil Engineering," vol. i., page 150, third edition: "It is common in this country to mix a small quantity of the coarsest sugar—'goor,' or 'jaghery,' as it is termed in India—with the water used for working up mortar. Where fat limes alone can be procured, their bad qualities may in some degree be corrected by it, as its influence is very great in the first solidification of mortar. Captain Smith attributes the fact that mortars made of shell lime have stood the action of the weather for centuries to this mixture of jaghery in their composition. He made experiments on bricks joined together by mortar consisting of 1 part of common shell lime to 1½ of sand. One pound of jaghery was mixed with each gallon of water with which the mortar was mixed. The bricks were left for thirteen hours, and after that time the average breaking weight of the joints in twenty trials was 6½ lb. per square inch. In twenty-one specimens joined with the same mortar, but without jaghery, the breaking weight was 4½ lb. per square inch."

Mr. Molesworth then adds: "The use of sugar or jaghery was known to me when I was in Ceylon twenty years ago. The masons who came over from Madras used to make most beautiful plaster work, almost like enameled tiles, of shell lime mixed with jaghery. The surface took a fine polish, and was as hard as marble; but it required a good deal of patient manipulation well suited to the national character."

This intelligence from India supplies proof of the most positive kind of the enormous strengthening power of sugar when mixed with mortar. It may be argued that some of our limes and cements are of themselves good enough without it. It is for engineers to judge whether they might not be made much better by it, or whether the facts I have brought forward do not show plainly that there should be an inquiry instituted by scientific men to investigate the actual *numerical* value of sugar, and the various conditions under which it acts, whether for better or worse. For the worse it cannot act, except such an insane use of it be made by adding too much, as to expect sugar to be itself mortar. The jaghery sugar used in India is sold in the London market at, I think, less than a penny a pound, and is used for feeding cattle. Treacle seems to me to be a most promising form of saccharine matter. I would shirk beetroot sugar. There is a rough, unrefined treacle which is very cheap, and I should suppose would be excellent. A halfpenny worth of treacle and water added to a hod of mortar would, I conjecture, increase its strength by one-third, if not by one-half. But I must leave the matter in the hands of scientific engineers. I think it is very probable that this use of sugar with lime is of extreme antiquity, and that a knowledge of it passed from India to Egypt and Rome; and that these nations used malt for its saccharine matter as a substitute for sugar. I have shown that the mediæval builders used beer in building our cathedrals, and beer is still used with plaster of Paris. These I take to be the remnants of ancient tradition. It is said that in the cold winter when Bess of Hardwicke died, her masons had to "melt the beer which they mixed with their mortar." They would have acted more wisely if they had used infusion of malt only, for most of the sugar must have been converted into alcohol, and lost for the purposes of mortar making. Antiquarians may be able, from old documents, to throw light upon this subject; but I strongly suspect that the old Roman mortar had saccharine matter added to it; and I am of opinion that in all engineering works requiring great strength it would be wrong not to take advantage of facts confirmed by the experience of ages.

Cranley, Surrey, November 30, 1886.

—*The Engineer.*

IMPROVED DARK ROOM LAMP.

By F. C. BEACH.

THE object sought to be accomplished by this lamp is to provide a diffused but strong non-actinic light for the convenience of the photographer, with a view of preventing any injurious effect upon the eyes. It is a gas lamp, and has been in practical use in the large dark room at the quarters of the Society of Amateur Photographers in this city.

FIG. 1.

The construction of the lamp is readily seen in the accompanying engravings. As shown in Fig. 1, it is a sheet iron box, about a foot square, suspended by wires from the ceiling, and connected by a stove pipe at the top to a chimney, for conducting off the waste heat.

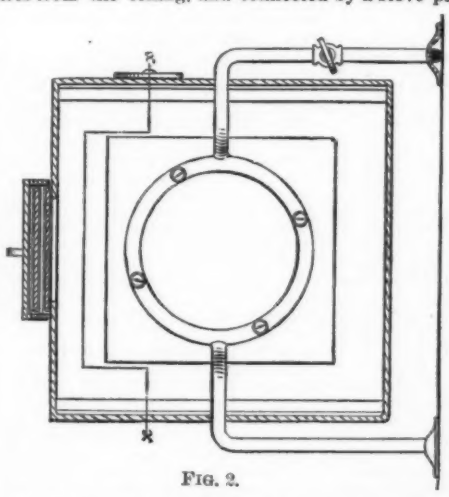


FIG. 2.

Perforations are shown on each side, shielded by L-shaped strips of metal on the inside (see Fig. 3), for permitting the free ingress of air, and preventing any escape of light.

The bottom of the lamp is closed with two panes of glass, one a cathedral green, the other a cathedral orange, having a sheet of canary yellow tissue paper between the two. Special grooves are made in the sides to allow the sheets of glass to be slid in or out (see Fig. 3).

There is also a small window of double glass on the front of the lantern, 4 × 5 inches in size, covered by a metal slide, the latter being pushed to one side, as shown in Fig. 1, when the operator wishes to examine a plate by transmitted light. A pivoted metal slide, round in shape, allows the lighting taper to be inserted for igniting the gas.

As shown in Fig. 2, the gas supply pipe enters one

side of the box, and is soldered to a tube ring about four inches in diameter, in which are four burners. The opposite side of the ring is supported by an iron tube, passing through the side of the lantern at right angles, and terminating in a bracket which is secured to the wall. A faucet on the outside supply tube regulates the flow of gas. Suspended below the gas ring is a square sheet of mica, for preventing the heat of the lights from radiating upon and cracking the glass beneath.

Instead of arranging the gas jets vertically, as shown

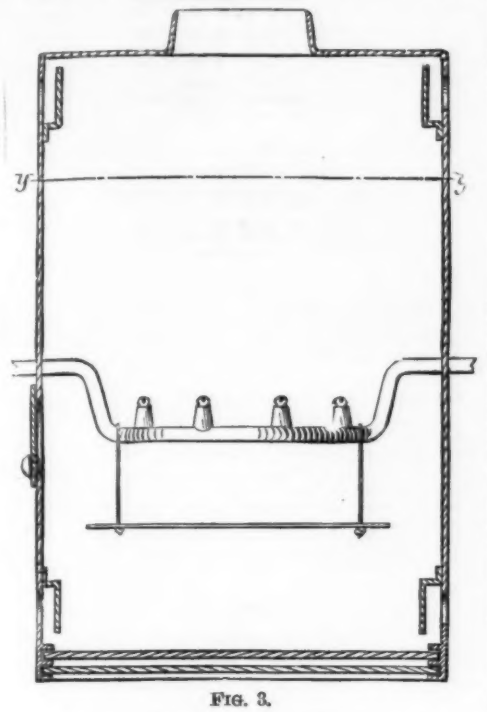


FIG. 3.

in Fig. 3, it is advised to place them horizontally, as in Fig. 4.

Fig. 3 is a sectional elevation of the lamp, showing the mode of suspending the mica sheet under the gas ring. The aperture at the front through which the two bottom glass sheets enter when inserted in place is protected by a long, narrow hinged metal door, to be plainly seen on the bottom front of the lamp in Fig. 1.

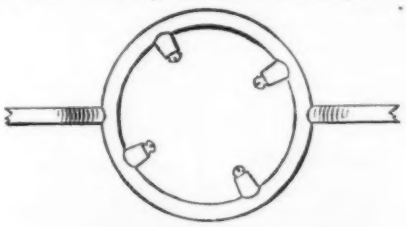


FIG. 4.

The lamp has the advantage of throwing the light downward upon the plate, just where it is most needed to clearly observe the operation of development.

HAND STAMP MILL.

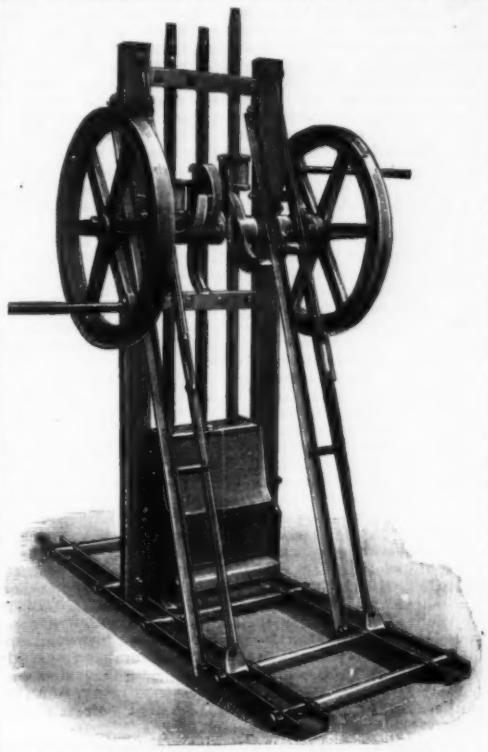
WE illustrate herewith a new design of gravitation stamp battery of three heads for the use of prospecting parties on newly discovered goldfields, the stampers being, as will be seen from the engraving, worked by hand power by means of flywheels with handles, which are fitted at each end of the cam shaft. The framework is strongly and simply constructed of wrought iron, and in less than half an hour the whole mill can be taken to pieces for transit and as readily re-erected and set to work in a fresh location; the total weight packed for shipment is about one ton, forming a convenient load for a bullock wagon or country cart, but the battery can also be provided with its own wheels if desired.

The stamper spindles are of steel, and the tappets are fitted with steel gib and crosskeys; the stamper heads and shoes are, in the smaller sizes, made in one piece, both shoe and anvil or die being of a special quality of forged steel; the guides for the stamper spindles are of hard wood, unless metal guides are preferred, and the batteries are generally sent out with copper sheets for tables or with blankets, as well as with an assortment of spare parts. The component parts are as few as possible, and are nearly all of wrought iron or steel, so that breakage and risks of loss in transit may be reduced to a minimum.

These little mills are in fact a reproduction on a reduced scale of the regular gravitation stamp batteries, that shown in the engraving being capable of crushing from 7 cwt. to 8 cwt. of quartz per day; so that, allowing for stoppages and the usual loss of time with work of this character, a prospecting party will get an actual "milling test" of from 26 cwt. to 30 cwt. of quartz within a week, and will thus be able to form a reliable opinion as to whether the reef under examination will pay for properly working.

It is, perhaps, hardly too much to say, especially in the case of the discoveries a few years since of reputedly auriferous quartz reefs, that if prospecting machinery of this kind had been sent out before permanent plant with its contingent expenses was decided upon, tens of thousands of pounds of the capital of the small investors might have been otherwise, and it may be more profitably, employed.

The hand stamper battery illustrated is the smallest usually made; larger mills of the same general design, with heavier stampers of three or five heads per mortar, are also arranged to work by hand power or by horse gear according to circumstances. Similar but heavier prospecting batteries to work by steam have in some cases the stamp mill, engine, boiler, etc., mounted on a self-contained girder bedplate of wrought iron or steel, so as to reduce the cost of and time occupied in preparing temporary foundations; and where there is a sufficient supply of water, the engine and boiler is often advantageously replaced by a turbine or waterwheel. Charles Appleby & Co., of 89



A PROSPECTOR'S HAND STAMP MILL.

Cannon Street, London, E. C., makers, have already a number of these batteries working successfully in the Transvaal goldfields and elsewhere.

THE HISTORY OF CALIFORNIAN BORAX.

By ARTHUR ROBOTTOM.

SIR EDWARD BULWER LYTTON, in the "Last Days of Pompeii," bears testimony to the value set upon borax in the days of the Roman republic. "Borax," says Sir Edward, "was largely used by Nero and his slaves, near 2000 years ago, and Pansa deeply regretted that he was not rich enough to buy borax to cover the arena after the death of the combatants at the time of the fight between Lydon and Tetraides." Many a time since my visit to the Californian borax districts has this passage sprung into my mind, and often I have thought what a pity it was that Olanthus, Glaucus, Caligula, and the gladiators did not know of the existence of the great borax lake in Slate Range Mountains, California.

My visit to this lake was one of the most interesting journeys I think I ever made in my life, and the accident of my making this journey arose in the following manner: In the year 1874 I visited the borax deposits in Nevada, and was on my way to San Francisco, when during the journey I was interviewed by a reporter of a Winnemucca newspaper, who, without my knowledge, sent on to San Francisco the following particulars concerning my visit in his paper, and which were duly reproduced before my arrival in the *Californian Alta* of San Francisco: "Mr. Arthur Robottom, of Birmingham, England, paid our town a visit a few days ago, on a prospecting tour, to discover if any borate of soda or borate of lime exists in this section. He was much struck with the number of hot springs that are to be found all through the Humboldt Valley. The borate of soda and borate of lime are known to exist at Hot Springs station, on the Central Pacific Railroad, also at Columbus and other parts of this State; and no doubt if the hot springs of Humboldt Valley should prove to give off vapors that produce boracic acid, or that borate of soda is to be found in the alkaline beds which exist all over the district, a new and profitable trade will spring up. The article of borax has been well known all over the civilized world; still but few people know anything of its origin. In 1818 Count Lardarel discovered how to prepare boracic acid from the lagoons of Tuscany, and made a princely fortune by it. This boracic acid was shipped to England and France, and converted into refined borax by boiling in large pans and crystallizing in vats. Tincal was the article used in making borax before boracic acid was discovered. This article is found on the dreary plains of Thibet, in Asia, and sent on sheeps' backs across the Himalaya Mountains to Calcutta, and thence to England. About 20 years ago borate of lime was discovered in Chili, and found its way to England, Mr. Robottom being one of the first to introduce it into that country. Mr. Robottom will also visit the Hot Springs, Wadsworth, Columbus, and San Francisco." The result of this having appeared was that, the morning after my arrival in San Francisco, my hotel was fairly besieged with persons who were anxious to interview me in my capacity as a borax expert. After many interviews with individuals who professed to hold land said to be rich in borax, I decided to visit and inspect the great Slate Range district, and, after making all my arrangements, I left San Francisco, proceeding southward by steamer down the Pacific coast, and after a pleasant journey arrived at Los Angeles, or City of Angels, an old Mexican town. Los Angeles is now, however, peopled principally with Americans, who drive a considerable trade with the teamsters who visit the interior. At this time the Southern Pacific Railway was not made, and this section of the country could be traversed only by the aid of mule teams. The Slate Range lay about 240 miles inland, and the whole country was infested with a band of ruffianly bandits and robbers, composed of the very refuse of society from all parts of California and Nevada. Under these circumstances, I soon discovered that my only chance of traveling with any safety was to assume the role of what is known in this part of the world as a "busted" miner, "busted" being a convertible term for ruined, and derived probably from "bursted," or broken up. I was also informed that, on some parts of the journey, hay was worth £50 per ton and water two shillings per bucket, and that it would be better for me to walk, and much safer. Adapting myself to the circumstances, I started in my disguise, and traveled with a mule team, over a very rough country, at the rate of from 12 to 14 miles per day, and arrived at length, without any remarkable adventure, at the shanty kept by Jim Bridges, some 42 miles from the Slate Range, and which is situated on the main road to Cerre Gorda, a wild-looking spot, without any other road, the country being covered with the oleaginous plant known as greasewood, and the only animal life being represented by the dismal owl and the deadly rattlesnake! Not a very pleasant prospect, I thought; but I afterward found, when sleeping out in this part of the country, that by surrounding myself with the ashes of a greasewood fire or a horsehair rope, as far as the rattlesnakes were concerned there was no danger, as these reptiles, strangely enough, will never cross the ashes of a greasewood plant or a rope made of horsehair. While, as to the owl, his existence was naturally a pure matter of indifference to me, so far as my comfort was concerned.

Rather a curious incident occurred to me while staying at Jim Bridges'. A pioneer and prospector had come from the mountains to get a few supplies from the store, when he saw me sitting on a bench outside, and after a few words had passed, he remarked, "Why, you are from the Old Country, and pretty green too! Whatever brings you into a country like this? You are no miner! Have you been writing somebody's name on a bit of paper, or done some act as you're afraid of the sheriff? Or are you on the wrong side with the other sex?" My reply was that I was all right on these points, and had come out here prospecting for borax. He at once begged me to have nothing to do with it. He said, "There is plenty of it, but no one knows what it's good for." He had been down to Death Valley, and to the foot of the hills of the Slate Range, through Owen's Valley, and to Mono Lake, and he gave me much information about this "howling wilderness."

After a short stay at Jim Bridges' shanty I again proceeded, steering for the Foot Hills, some 22 miles from the shanty, then onward through a great canon, or divide, partly covered with salt, on emerging from which I found myself on the border of the most important borax lake yet discovered in the world.

I was met by John and Dennis Searle, two men belonging to the California discovery army that sprang into existence in the year 1849, and whose members are known by the name of "Forty-niners." These men, masters of almost every kind of handicraft, had made their way to this great lake with a view to exploration. Consequently, though I can claim to be the first Englishman who visited the borax lake, the honor of its discovery does not rest with me. I stayed some time in the hut of these men, and together we examined the ground. I very soon discovered natural borax of the finest quality in a pure state, and though Messrs. John and Dennis Searle had begun prior to my arrival to develop the ground, the first shipment was made by me to England. The borax I found was crystallized borax, in the same form as the regular borax of commerce, and is the only known deposit of natural borax yet discovered in the world. In the center of the lake is a bed of salt about five miles long. On the outside of this salt is a deposit of carbonate of soda, and some thousands of acres of land covered with crude borax from three inches to two feet thick. The crude borax is collected and put into cowhide baskets, carried to a large boiling pan, and boiled for 36 hours; the solution is then run into vats and the crystals form on the sides of the vats. After drying it is put into bags, about 70 lb. in each bag, and sent to San Francisco, a distance of about 420 miles, and conveyed at that time by mule teams. Before leaving California I arranged to buy 1,280 acres of this borax land. I returned to England as quickly as possible, made arrangements to go out again, formed a small company, who put up works, and I anticipated making about a million by it, and before long we began to ship large supplies of borax to Liverpool, London, and New York. The price, however, suddenly came down to £20 per ton, the lowest price it had ever been sold for, while the carriage alone from the lake to San Francisco at this time was about £16 per ton of 2,000 lb. This was a paralyzing condition of affairs that quite stunned me. I had made a very large contract for some hundreds of tons to a large firm in England, but before the arrival of the ship with the first parcel the firm failed, and I found myself, to my horror, with some hundreds of tons of borax left on my hands that I could not get rid of, the bankers and financial houses holding the documents all pressing for sales to be made! My golden dream of making a million faded away, and left me meditating on the bitter realities of impending ruin! (I yet anticipated that this property will at some future period be worth £1,000 per acre. Experience has proved that the crude borate of soda on the surface of the land reproduces itself every three years.) Assoon, however, as I had somewhat recovered from my shock, I began to realize that I must do my utmost to find a market for the borax. I was driven to every move that a man could suggest to try and begin to get clear of the stock. I knew that if the public could only be made to understand the true merits of the article, and would begin to try it, all would come right for myself and friends. Consequently I set to work, and began to have the ground borax packed in penny packets. Here again I met with disappointment, for I soon found out that the public are very slow in taking to a new article, and I could not induce the retail shopkeepers to sell it. I offered it to the druggists. They all replied, "You want us to sell far too much for a penny. We want eightpence for what you want a penny for;

25 per cent. profit won't pay us." I then tried the grocers. They were satisfied with 25 per cent. profit, or less, but their reply was as characteristically disheartening. "It's all right," they said, "but borax is some kind of medicine, not a grocer's article; we can have nothing to do with it—it's too much trouble to tell the customers its merits." The whole business began to look very dark and gloomy. Almost in despair, I took a stand at the first dairy show in the Agricultural Hall, and met with marvelous success. I then tried many of the laundries in and about the suburbs of London, and I also tried many of the hospitals and other public places, with more or less success.

A lady universally known from her philanthropy sent her almoner to see me about it, and a specimen was sent by her desire to one of the first scientists of the day; but the true merits of borax were not then known, even to the wisest of the wise. My belief in the borax, however, has never deserted me. I made many experiments and collected much information as to its uses, and the consumption during the last few years has very considerably increased; but this has only occurred since I parted with my interest to the Patent Borax Company of Birmingham, who have naturally been enabled to bring the product more forcibly to the front as one of the most useful and important products yet discovered in the world. I still feel a very keen interest in the future of this product, though I have no pecuniary end to serve in saying so; but the multiplicity of its uses is truly marvelous, and the following information cannot, in my humble opinion, have too extended a circulation. For the laundry there is a very great deal of labor saved by using borax, as this product "softens" the dirt, and the latter may almost be said to float out of the linen, saving at once the destructive friction and the time and materials. For the household its uses are legion. Beds, etc., washed with a strong solution of borax in the spring, prevents insect life from forming; dusting the floors with dry borax destroys the larvæ of the moth, and keeps the carpets entirely free from insect life. For cleaning marble, plate, jewelry, decanters, plates; for improving the flavor of boiled vegetables, tea, etc., and preserving eggs, fish, butter, and milk, borax, in one or other preparations, is simply invaluable. It is the best tooth wash known; will clean brushes, sponges, etc., from dirt. Medicinally, the value of borax is not as fully known as it should be. I hold letters from eminent medical men who have corresponded with me upon its value, and who esteem it highly. The *Lancet* of May 20, 1876, contained a laudatory article on it as a valuable antiseptic, "which does not irritate and inflame." I have cured thousands of persons suffering from sore throat, by giving them a small piece of borax to suck (I always carry a bit in my pocket wherever I go); and for dissolving the phlegm and clearing the throats of speakers, my own experience proves to me that it is the only reliable remedy.

Since the first publication of the above, extensive progress has been made in the development of the uses of borax by the general public; and from letters, the thanks and encouragement I am receiving from many friends and strangers satisfy me that I have given information which well repays me for all the trouble, losses, and the inconveniences I have been put to in trying to make this wonderful production better known.

Touching the labor uses of borax, it is mainly used in glazing all descriptions of porcelain, china ware, pottery, etc. Blacksmiths use it for welding iron and steel. It is used for welding the seams of copper and iron tubes; in the manufacture of hats, jewelry, artificial diamonds, and the plates for affixing artificial teeth. The finest marble cement is made from borax. Farmers, graziers, etc., use it for washing cattle; and provision merchants for arresting or preventing decomposition in their hams, etc. The foregoing will, I trust, awaken people to an interest in this most valuable product of the earth.

Native Borax.—Analysis.

Sodium biborate—pure	99·75
" chloride—trace only	0·25
	100·00

The following are the rates of wages paid for labor in this section:

	£	s.	d.	
Blacksmiths	1	0	10	per day.
" helpers	0	12	6	"
Engineers	0	16	8	"
Teamsters	0	13	6	"
Coopers	0	13	6	"
Boilermen	0	10	6	"
Watchmen	0	10	6	"
Laborers, principally Chinamen	0	5	3	"

The Southern Pacific Railway Station is only about 72 miles from the lake, where the borax is carried by 20 mule teams, but in about three years or less a railway will run within four miles of this desolate country, when these wonderful natural deposits will be more carefully examined by scientific men.

Before concluding this article, I may here state the borax lake is not a very enviable place to live in. On my first visit, Jim Bridges' shanty (42 miles from the lake) was the nearest place from which we could obtain our coffee, sugar, or canned fruits, etc., and post our letters. We had no drinkable water within 17 miles. Always a clear blue sky (a little London fog would have been a great treat). Not a tree visible; no vegetable, only the oleaginous greasewood plant. No animal life, no Indians, no clouds, no rain; and last, but not least, a total absence of the fair sex. The freight alone to build our works cost 2½d. per lb. from San Francisco.—*Chemical News.*

THE measuring of the candle power of a light is accomplished by comparing the shadow cast by a rod in the light of a standard candle with the shadow cast by the light to be tested. By moving the latter toward or away from the rod a point will be reached at which the shadow cast by both lights will be of the same intensity. The intensity of the two lights is directly proportional to the squares of their distances from the shadow, *i. e.*, suppose the light to be tested is three times the distance of the candle, its illuminating power is nine times as great.

[Continued from SUPPLEMENT, No. 572, page 9139.]

FRICTION.*

By Professor H. S. HELE-SHAW.

Lecture II.—Delivered January 25, 1886.

THE NATURE AND LAWS OF THE FRICTION OF FLUIDS.

THE motion of fluids has received as much attention from eminent mathematicians and physicists as perhaps any other subject except astronomy, and yet it is only two hundred years ago that the existence of what is in some respects their most important property was first pointed out. In 1686, Mariotte's "Traite du Mouvement des Eaux" was published, in which work the discrepancy between results calculated by theory and results obtained in practice was attributed for the first time to the fact that there was such a thing as fluid friction. The Italian engineer Guglielmini had a short time previously attempted to explain the retardation of flow in rivers by assuming that the obstructions in their beds deflected upward small bodies of water, which, moving at first with a velocity less than that of the general mass of water, received an increase of velocity from that mass, and so caused a reduction in the mean rate of flow. Mariotte showed that in glass tubes the discrepancy between existing theories and practice also occurred, where he supposed such obstructions could not exist, and concluded that the retardation was really caused by friction against the sides of the channel in which the fluid flowed, and that this was communicated by friction to the contiguous particles, and so on throughout the whole mass.

Since that time innumerable experiments on the flow of fluids have been made, and many treatises written on the subject, and yet at the present day we cannot definitely say what exactly is the nature of that action which always dissipates the energy of fluid motion. Still, however, we know that in certain cases both the causes suggested respectively by Mariotte and Guglielmini come into operation together, while in every case there is some action like the resistance of friction corresponding to that which we were led in the previous lecture to conclude must subsist between the parts of the solid bodies. In addition to the above two causes there is a third, which, although more difficult to trace, we believe, from what we know of phenomena of diffusion, must come into operation, and in the case of gases to an important extent. The subject is, however, one of such difficulty that I propose to commence the study of it in a very simple manner, and discuss the flow of a perfectly incompressible fluid, that is to say, of a frictionless liquid, by means of the so-called "stream line" theory.

We shall be compelled to admit that the motion of actual liquids is accompanied by some action corresponding to the friction of solid bodies. We shall then proceed to consider what is actually known from experiment about the motion of liquids, so as to be able to understand the laws of fluid friction, and finally deal with the case of gases.

In the first place, let us consider what takes place in the passage of a liquid through bent pipes or channels of uniform section. Let i, Fig. 22, represent a smooth

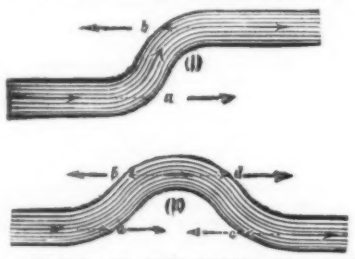


FIG. 22.—FLOW IN CHANNELS OF UNIFORM CROSS SECTION.

pipe in which the perfect fluid, moving in the direction of the arrows, is deflected round a double bend, and finally moves parallel to its original direction.

The result of this is that at each bend there is an oblique reaction, the components resolved parallel to the original direction being equal and opposite for each. Therefore there is no resultant tendency to move the pipe bodily along, but only to turn it in a direction contrary to that of the hands of a watch. The dotted arrows in the other figure (ii) show that in passing round a second double bend the resolved parts of the reactions parallel to the original direction all balance each other, and as this is also obviously the case with the two equal and opposite couples, it is clear that the liquid has returned to its original line of flow without any changed velocity, and without having any resultant effect on the channel in which it has flowed. The same mode of reasoning leads us to the conclusion that the velocity of a stream of this kind is unaffected by the form of the channel, however tortuous, in which it moves, and, conversely, that the channel itself is unaffected by the passage of the stream through it.

Next let us consider the effect of changes in the cross section of the channel. Let Fig. 23 represent digrammatically particles or groups of particles moving at the rate, say, of 3 feet a second, then we may imagine 12 of these (shown in black) to pass a certain point each second. We may now suppose that these 12 groups of particles arrive at a portion of the channel where the sectional area is half that of the former case. The fluid being incompressible, they must pass this point in the same time as before, and they can only do this by increasing their velocity. If, for instance, they are 1 foot apart, and the former channel was 4 feet, they would formerly occupy a length of 3 feet. Now, the channel being 2 feet wide, they occupy a length of 6 feet, as shown in Fig. 24, and consequently, in order to pass a given point in one second as before, they must move at the rate of 6 feet a second, or double their former velocity. That is to say, the rate of motion of a liquid varies inversely as the area of cross section. Applying this to the case of a channel (Fig. 25) of varying cross section, we see that from the section (*a a*) to the section (*b b*) the velocity is increasing, and as this can only be obtained by an access of pressure from behind, the pressure must be decreasing up to the latter point.

* Lecture recently delivered before the Society of Arts, London.

From the section (*b b*) to the section (*c c*) the velocity is decreasing and the pressure is increasing, but the channel having returned to its original cross section, the pressure and velocity are again the same at this point. Thus it is clear that changes in the cross section of the channel have no effect whatever upon the final rate of flow when the cross section is the same as it originally was.

FIG. 23.

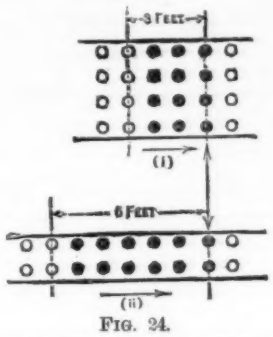


FIG. 24.

DIAGRAM SHOWING THAT THE VELOCITY OF FLOW VARIES INVERSELY AS THE CROSS SECTION OF THE CHANNEL.

It is now easy to understand the stream line theory of the motion of fluids and through fluids. Fig. 26 represents a channel in which an incompressible fluid is moving in the direction of the arrows. The lines in the section represent stream lines bounding imaginary streams of fluid; and it will be seen that, reasoning upon any particular stream between any two lines, the final condition of the fluid is the same as it originally was, and therefore there is no change of velocity in any of the separate streams, and consequently none in the

FIG. 25.—FLOW IN A CHANNEL OF VARYING CROSS SECTION.

whole body of liquid flowing in the channel. That is to say, the section at *a a* being the same as at *c c*, the conditions of flow in the two places are identical, and no loss of energy has taken place in passing the contracted section, *b b*. In exactly a similar way, we may show that, if the body shown in Fig. 27 be moved through the fluid, the original state of the imaginary stream lines being again restored, there is no energy imparted to them by the moving body, and consequently there is no resistance whatever to the motion of the body itself.

FIG. 26.

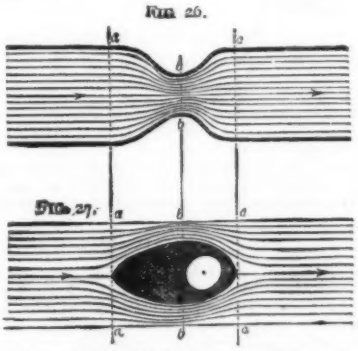


FIG. 27.

THE "STREAM LINE" THEORY.

We know well that in practice there always *is* a retardation of the flow of liquids, even in straight pipes and channels, and that when there are bends, even with continuous curves the retardation is considerable. So, likewise, in the case of bodies moving through a liquid, we know that the resistance to motion is considerable, even when—as in the case of well-formed

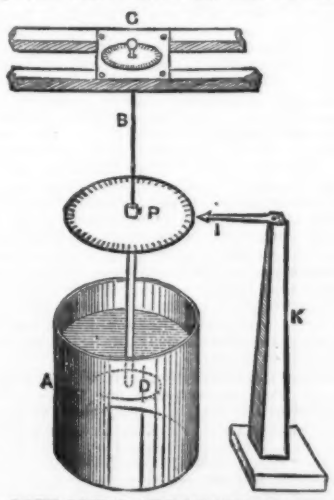


FIG. 28.—COULOMB'S APPARATUS FOR STUDYING THE FRICTION OF LIQUIDS.

ships—the form is what is called "fair." Thus, now that we have made it clear that no resistance whatever to the motion of a perfectly frictionless fluid could in such cases occur, we may conclude that no such substance exists, and that there is an action of the nature of friction in fluids. We will now proceed to consider what is actually known of the exact nature and amount of the frictional resistance.

The earliest experiments on fluid friction were made about 100 years ago by Coulomb, whose apparatus consisted of a disk, D (Fig. 28), suspended by a brass wire, B, so as to be able to oscillate about a vertical axis in the vessel of liquid, A. The extent of the oscillations could be read upon a dial, P, by means of an index, I, supported on a stand, K. Thus by noting the time in which the disk came to rest in a liquid, and comparing with that in which for a similar initial displacement it came to rest in the air, it was possible to estimate the frictional resistance of the liquid to the motion of the disk.

Coulomb's experiments, although made at very low velocities, and leading him in one particular to an erroneous generalization, proved conclusively that the laws of fluid friction were of an opposite nature to those of solid friction, and their method of statement is thus, as we shall see, entirely different. Since the time of Coulomb, data and results on the subject have been continually accumulating, such as those of Colonel Beaufoy, but without doubt the most important experiments that have been conducted are those by the late Mr. William Froude. Mr. Froude's apparatus consisted of a stout truck running on a pair of rails above a large covered tank, 278 feet long, 36 feet broad, and having for the most part a depth of 10 feet. From the truck could be suspended bodies of any desired material, having any required form, and the truck being drawn along the rails by a steam engine at a rate which could be accurately regulated, the body was at the same time towed through the water beneath. The resistance of the water to the passage of the body through it, and also the rate of motion, were automatically recorded on a revolving cylinder.

The experiments of Colonel Beaufoy and Mr. Froude were made with solid surfaces of limited area in water of practically unlimited extent. Professor Unwin has recently published the result of experiments made by him with surfaces of virtually unlimited extent in a limited volume of water. He accomplished this by rotating a disk in a vessel of water, and employed the apparatus shown in elevation and plan, Figs. 29 and 30.

FIG. 29.

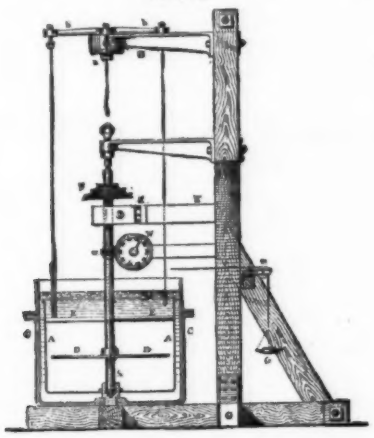
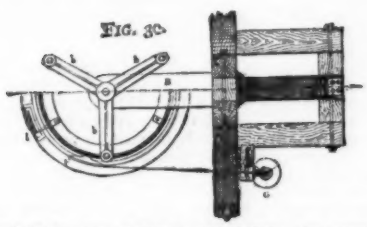


FIG. 30.



APPARATUS OF PROF. UNWIN IN EXPERIMENTS ON THE FRICTION OF DISKS ROTATED IN FLUID.

This apparatus consisted of a strong frame carrying a vertical spindle, S S, which could be turned at various speeds by a pulley, P, driven by a band from a hot air engine. Upon this spindle disks of different sizes and various kinds of material could be fixed, so as to be within the cylindrical vessel of water, C C. The resistance to rotation was measured, not by the torsion on the spindle, but by the effect of the inclosed volume of water upon a cylindrical casing, A A (the effectual depth of which could be varied by means of a movable disk, E E), suspended by three wires to a frame, *b b b*. The scale pan, G, suspended over a pulley (*e*) to the edge of the casing, served to measure this rational effect, while at the same time the velocity of rotation was observed by means of a speed indicator.

All these experiments which have been described were made with surfaces moving in a fluid, but concurrently a large mass of data upon the flow of water in a pipe or channel had been obtained, culminating in the elaborate and complete experiments which were carried out by M. Darcy, of the Paris Waterworks.

We can now discuss the nature of the results obtained by the various experiments, and it may at once be said that the friction between a solid and a liquid differs from that between two solid bodies in that it—

1. Is *independent* of pressure.
2. *Does* vary with the extent of the surfaces.
3. *Does* increase with the velocity of motion.

These results are mathematically expressed thus:

If F = the frictional resistance,
S = the extent of the surfaces in contact,
V = the velocity of relative motion,
Then $F = f S V^n$

where f and n are quantities both depending on the velocity of motion and also on the nature of the surfaces. And what is required is to find under the various conditions of practice suitable values for f, or the "coefficient of friction" as it is called, and for n, or the power of the velocity to which the resistance is proportional.

Coulomb, as the result of his experiments on the motion of a body in a liquid, supposed that the amount of the resistance was independent of the nature of the surfaces and varied directly with the velocity, that is to say, that f was unaffected by the kind of surface employed, and n was equal to unity. Curiously enough, the earlier experiments upon the effect of surface in the case of the flow of water in pipes seemed to give the same result. The experiments of Beaufoy and Froude on the one hand, and of Darcy on the other, showed, however, that, except for very low velocities, these conclusions were quite erroneous. In the first place, it is now known that the roughness of surface has considerable effect in retarding fluid motion over it. This is well illustrated by the results which Professor Unwin has given, in the table below, to show how closely his experiments with the rotating disk agree with the results obtained by Mr. Froude upon plank 50 ft. long. Professor Unwin has plotted his results in the form of curves (Figs. 31, 32, and 33), in which each division horizontally represents one revolution per second, and each division vertically a resistance of one pound, acting at a leverage of a foot on the suspended cylinder which inclosed the rotating disk. Fig. 29 shows the results with a cast iron disk, the dark line being that with the space in which the disk rotated, adjusted so as to be only 1½ inches deep, the other curves representing those for 3 inches and 6 inches respectively. Fig. 30 shows the results with a bright brass disk, in which case the mean frictional resistance is obviously lower with the bright brass than it was with rough cast iron. Fig. 31 shows the result of four experiments with a rough and smooth disk, each in a rough and smooth chamber. The curves are exactly as would be expected, the highest being that for the two rough surfaces, the lowest for the two smooth ones, while the second highest is for the rough disk in the smooth chamber, and the third for the smooth disk in the rough chamber. With regard to friction in channels, Darcy found that the resistance in clean cast iron water pipes was 1½ times as great as with a lining of pitch, while for old and incrusted mains it was twice as great as with new cast iron.

RESISTANCE IN POUNDS PER SQUARE FOOT AT A VELOCITY OF TEN FEET PER SECOND.

Froude.		Unwin.	
Tinfoil surface.....	·232	Bright brass...	·202 to ·229
Varnish	·226	Varnish	·220 to ·233
Fine sand	·337	Fine sand.....	·339
Medium sand	·456	Very coarse sand	·587 to ·715

FIG. 31.

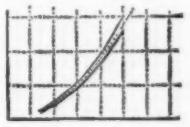


FIG. 32.

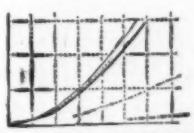
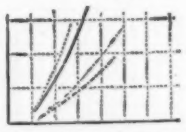


FIG. 33.



CURVES REPRESENTING THE RESULTS OF EXPERIMENTS UPON FRICTION OF DISKS IN FLUID.

These general facts are not, however, sufficient of themselves to enable us to determine suitable values for the coefficient for friction, f, which also depends in some way on the velocity. The fact is that, in spite of many attempts, it has been found extremely difficult to obtain such values so as to include both high and low velocities, and there is now reason to believe that this involves an important physical fact, which we shall presently allude to, and which would account for there being no single value of the coefficient of friction applicable to all cases of the motion of fluids.

The search after some form for the various coefficients of friction for the flow of water has absorbed a very large amount of time and labor on the part of the various authorities on hydraulics, for it must be remembered that we have only been considering the value for the retardation which takes place at a known velocity. There is also the case of the frictional resistance to increase of velocity; for instance, in the case of a river with a given surface slope. Here it is important to calculate what the velocity will be, and this might be at once done if the proper coefficient of friction were known.

It has been remarked that Coulomb found for low velocities the value of the power of the velocity, n, was unity. This was for velocities of about 1 inch per second. At velocities of 6 inches per second, the power was afterward found to be more nearly equal to 2, that is to say, the resistance varied more nearly as the square of the velocity. Or more simply, while for low velocities the resistance doubled if the velocity doubled, and became three times as great when the velocity trebled at higher velocities the resistance quadrupled as the velocity doubled, and became nine times as great when the velocity was trebled. Here, again, there was no exact value of the constant which was suitable to all cases, but the results of experiments of Mr. Froude and Professor Unwin agreed in a very remarkable manner, the value of n being in both cases, for smooth surfaces 1·85, and for rough surfaces from 1·9 to 2·1.

Now, what do the foregoing facts and laws of liquid friction reveal as to the nature of the resisting agency? It is certainly a remarkable fact that, after a certain velocity, the law of resistance should change in such a marked manner; and this particular point has been investigated by Professor Osborne Reynolds for the flow of liquids in pipes. There appears to have previously been no exact determination as to the exact velocity when the change in the value of n from 1 to 2 (or nearly 2) took place. Professor Reynolds experimented with color bands in glass tubes (Fig. 34) about 4' 6" long, and fitted with trumpet mouth piece, *m*, so that the water might enter without disturbance. The tubes were immersed in a tank, and the water was drawn through them, together with a streak of color from a central tube, *s*. The results obtained were that, up to a certain velocity, the parallel and uniform motion of outer and inner water was maintained, as shown in Fig. 34, i, and the colored band extended uniformly through the whole tube. At a certain velocity, and at a certain point of the tube, however, the band always broke up, and assumed the appearance shown in Fig. 34, ii. On viewing the tube with an electric spark, it was found that the band had broken up by the formation of eddies throughout the disturbed portion (Fig. 34, iii).

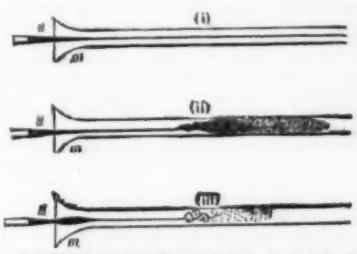


FIG. 34.—EXPERIMENTS WITH COLOR BANDS IN TUBES TO DETERMINE CRITICAL VELOCITY (PROF. OSBORNE REYNOLDS).

The general conclusions which seem to result from the various experiments made are that there is a critical velocity at which water in motion breaks up into eddies, this critical velocity, though varying between comparatively wide limits, distinctly marking the change when the resistance became proportional to a higher power than unity. These results afford strong grounds for supposing that the dissipation of energy, when occurring to any appreciable extent, takes place by the setting in motion of masses of water in the forms of whirls or eddies. This is not only so in the case of abrupt changes of direction of flow, such, for instance, as shown in Fig. 35, or where a submerged body has not a fair form, but also on continuous and smooth surfaces, as we saw in the case of the glass tubes. Such eddies could, however, neither be formed or dissipated without some kind of action corresponding to that first conceived by Mariotte. And we now know that, as it has happened before in similar cases, Mariotte and Guglielmini were to a certain extent both right in their views, although both took into account only one kind of effect. Curiously enough, it was from experiments upon glass tubes—in which, as we have seen, eddies occur in such a marked manner, but which eddies Mariotte apparently never took proper means to detect, and which he supposed could not possibly occur in connection with so smooth a surface—that he was led to conclude that the resistance taking place must arise entirely from the friction of contiguous particles. Just as in the case of solids, we do not know what is the ultimate nature of internal fluid friction, but we have a definite theory as to the property of fluids by which these particles are brought into contact with each other, which we shall consider when we come to discuss the friction of gases. Arguing from this property, the resistance of two layers of liquid to motion over each other must vary directly with the velocity of their relative motion, and this would account for the results obtained by Coulomb at very low velocities. When masses of water are set in motion in the form of eddies, the resistance may be shown to increase from the cause as the velocity increases. Thus from the combination of the two causes which act together, it is easy to understand why the frictional resistance at higher speeds increases at the higher power of the velocity.

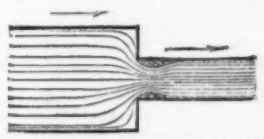


FIG. 35.—FRICTION CAUSED BY EDDYING MOTION IN A LIQUID.

The results which I have quoted have been selected with the view of illustrating the properties of liquid friction, and we must for want of time pass over the important facts connected with the flow of rivers, the scouring action on their sides and bed, the data concerning variations in velocity of flow obtained by gauging operations, the formation and transmission of waves, the flow of water over notches and through orifices, the flow of semi-solid fluids, such as tar, and must pass on from the question of the "flow" of liquids through or around solid bodies to another aspect of liquid friction.

We have hitherto considered only the results obtained with solid bodies and fluids, and have referred to such effects as "fluid friction." We are evidently justified in doing this, because the surface of the solid is invariably wetted, that is, the liquid adheres to it, and consequently it must be the resistance in the liquid itself, and not that directly between the solid and liquid, that retards the motion. This explains the fact observed by Coulomb that at very low velocities the nature of the surface of solids apparently does not affect the amount of the resistance to their motion in a liquid. A number of facts concerning the relative motion of fluids in contact with each other might be given, and also an account of various experiments, such as those upon drops, by Mr. Worthington, all of which entirely confirm the general conclusions at which we have arrived.

We have seen that the laws of solid and fluid fric-

tion are very different; but as long as we have the case of a solid moving in any considerable amount of liquid, or of a liquid through fixed surfaces, as when flowing in a channel, we have only fluid friction. When, however, the quantity of liquid between the two moving surfaces of a solid is sufficiently reduced, it is obvious that the effect of the surfaces on each other must be felt, as, for instance, in the experiments of Professor Unwin, where the result of reducing the area of the chamber in which the disk rotated by lowering the plate, E E (Fig. 29), was (contrary to what might be expected) to reduce the extent of friction, the larger area of surface in the former case causing more friction. There must be some point as the solid surfaces approach still nearer to each other where, since the laws of solid or fluid friction are of an entirely opposite nature, neither can hold. Such a case is that of lubricated surfaces. In the use of lubricants, the amount of fluid between the surfaces may vary from the case of an oil bath to that of a bearing almost quite dry. Thus we might readily expect to find a considerable variation in the laws regulating the friction of lubricated surfaces, according to the nature of the lubrication, and we should certainly, in view of the two sets of laws which would respectively regulate these two extreme cases, not be prepared to exclusively employ either set of laws without very careful examination. Morin did experiment upon this point at considerable length, but his experiments did not by any means extend over a range sufficient to embrace the conditions of practice now common. He concluded that the laws of solid friction were applicable to lubricated surfaces, and that "with unguents, hog's lard, and olive oil interposed in a continuous stratum between them, surfaces of wood on metal, wood on wood, and metal on metal (when in motion), have all of them very nearly the same coefficient of friction, the value of that coefficient being in all cases included between 0·07 and 0·08." The result of this has been the use ever since of the simple formula of solid friction with either Morin's value of the coefficient of friction or some other. It has, however, long been suspected that this formula was not applicable. M. Hirn, for instance, thirty years ago,* said that—

(1.) There exists a great difference between the phenomena which the friction presents of two pieces which slide over each other, and which are either dry and in immediate contact, or are separated by a bed of lubricating material (oil grease, water, or air).

(2.) In the case of friction which I call *immediat* (in which the two surfaces are dry), the coefficient of friction, otherwise the ratio of pressure and motive effort, is independent of speed, of surface, and of weight.

(3.) It is not thus for friction which I have called *medial* for those cases where the surfaces are separated by an unctuous material. Here the coefficient of friction is always a function of the speed, of the charge (load), and of the extent of the surfaces in contact.

M. Hirn makes several other statements which are interesting, and bear on the subject, but which I need not quote. The whole question has been in recent years the result of elaborate investigation, but before proceeding to give a summary of the results we must make clear the puzzling differences in the employment of the term "coefficient of friction." The term as employed by M. Hirn for the ratio of resistance of friction and load is used in the same sense as we have always used it for solid or *intermediate* friction. For fluids, however, the term we have used signifies quite a different thing from this ratio, and is in fact simply a convenient coefficient in the expression for frictional resistance. Now, for lubricated surfaces the authority of Morin led to the use of a constant value for this ratio. It soon became evident from experimental results that the ratio was not constant; and instead of seeking a convenient coefficient in a new expression, the idea has been in the case of some experimenters to find what values of the coefficient would fit the old expression under different conditions, and in the case of others to find a new simple empirical expression, but still to have a variable coefficient. M. Hirn employs the term as being a variable quality in the former sense, and it is very easy to express the relation between his value of the coefficient and the one we have employed for fluids.

Thus if f = the ordinary coefficient of fluid friction,
μ = the coefficient as used by Hirn,
R = the pressure between the surfaces

Then $\mu = \dfrac{f\,S\,V^n}{R}$

Coming now to the actual experiments, we find M. Marcel Deprez† gives an account of experiments made with an electro motor, in which he finds the coefficient of friction (taken as $\mu = \frac{F}{R}$) varies from 0·005 for a load of 145 kilos. to 0·025 for a load of 350 kilos., giving a mean result of 0·013. Here not only are the views of Hirn borne out, but the coefficients were much lower than those given by Morin, and we were led by reasoning to expect such should be the case.

The most important experiments have been those made by Thurston and Woodbury in America and Beauchamp Tower in this country. We can only give a brief reference to the results obtained, which are quite what might have been expected. Thus Mr. Tower says that "his own results, speaking generally, were so uncertain and irregular that they may be summed up in a few words. The friction depends on the quantity and uniformity of distribution of the oil, and may be anything between the oil bath results and seizing according to the perfection or imperfection of the lubrication. The lubrication may be very small, giving a coefficient of $\frac{1}{100}$, but it appeared as though the lubrication could not be diminished and the friction increased much beyond this point without imminent risk of heating and seizing." With an oil bath the coefficient could be reduced to $\frac{1}{1000}$ (or 0·001), and in one set of experiments the oil bath worked with a coefficient of friction which was $\frac{1}{4}$ of that in the case of a bearing very slightly lubricated.‡

Mr. Tower's experiments were made with comparatively high pressures, and most of them at a uniform temperature, but Mr. Woodbury has obtained a number of results at varying temperatures, with pressures ranging from 1 to 40 lb. on the square inch. With these light loads the resistance decreases as the temperature rises. Fig. 36 shows the curves obtained by plotting the results of a large number of experiments. Each curve is drawn for a constant pressure, that to the extreme left being the curve for 1 lb. per sq. in., the next for 2 lb. per sq. in., and so on up to 10 lb. per sq. in., the last four curves being respectively for pressures of 15 lb., 20 lb., 30 lb., and 40 lb. per sq. in. The points on every curve are found by setting off the coefficients of friction horizontally, and the corresponding temperature vertically. The curves show a tolerable uniformity in the results, and indicate that the friction increases with the pressure, also that it decreases with the rise of temperature, but nevertheless they could not be represented by means of any simple formula.

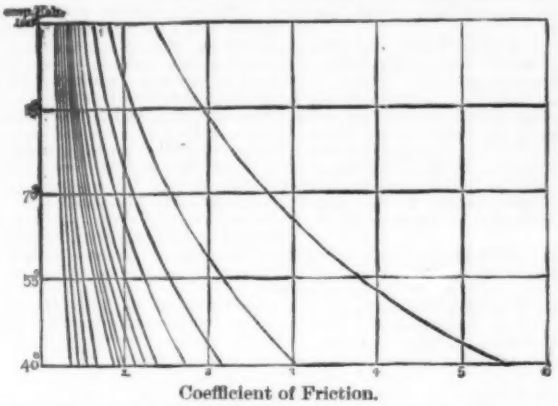


FIG. 36.—MR. WOODBURY'S RESULTS FOR FRICTION AT VARIOUS PRESSURES AND TEMPERATURES.

Professor Thurston's results, like those of Tower, show great variations, and make it evident that it is

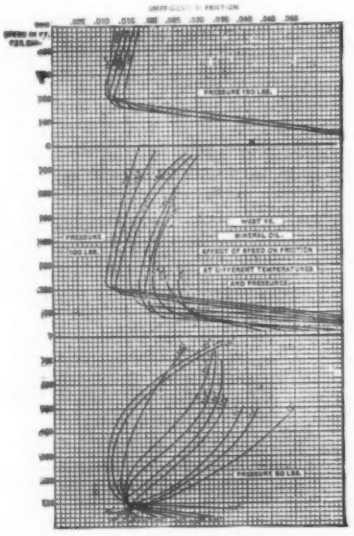


FIG. 37.—EXPERIMENTS BY PROF. THURSTON UPON THE FRICTION AT DIFFERENT VELOCITIES, TEMPERATURES, AND PRESSURES.

extremely hard to express results in a mathematical form, even when they are obtained in special experiments under the most favorable conditions. Thus take the results of three sets of experiments made at respectively 50 lb., 100 lb., and 150 lb. pressure on the square inch. The curves so obtained are plotted as in Fig. 36, each one corresponding to one definite temperature (marked upon it), the coefficients of friction being measured horizontally, and the velocities vertically. A simple inspection of these curves shows that, although the general result is that the friction at first decreased as the velocity increased, and then afterward began to increase, yet any attempt to represent them mathematically would, if it could be done at all, lead to a formula altogether too complicated for practical use.

I have not previously referred to the connection between the temperature of the surfaces and the resistance of friction, and the reason is now clear, for the law of variation of friction with temperature cannot be stated definitely. Mr. Tower's results, however, indicate a great diminution of friction as the temperature rises. Thus with 430 revolutions a minute, the friction was only one-third of the amount at 120° F. that it was at 60° F. Also, though there is evidently some direct connection between the temperature and velocity of motion, it cannot be accurately expressed.*

In practice there are so many disturbing causes which it is impossible to control, that it would seem as if we must be content with general deductions, and use a method of computation, where it is necessary to do so, that will embrace the most unfavorable case. Thus there is some justification for continuing to use the century-old expression ($F = \mu R$). The margin allowed in text-books and elsewhere seems certainly estimated to be on the safe side.

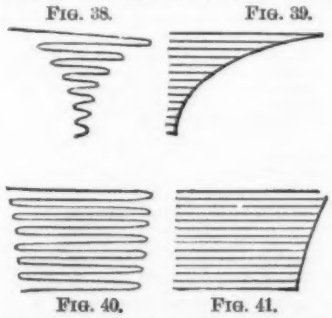
Thus in the Science and Art Examination of 1872 there was a question on the work absorbed by a flywheel of 20 tons weight, with an 18″ shaft. The coefficient of friction was given as 1, and at a velocity of, say, 60 revolutions per minute, this would necessitate a special expenditure of 4 horse power to merely turn that wheel in its bearings; whereas, by a properly designed and lubricated bearing, $\frac{1}{12}$ of a horse power should be sufficient.

There are many other points concerning the friction of lubricated surfaces, but the general nature of the action taking place has been now made clear, and the practical questions arising from its study must be dealt with in the fourth lecture.

The friction of compressible fluids or gases must now be considered. The friction of gases is so much less than that of liquids, that for most practical purposes its amount can be neglected. In recent years, however, when pneumatic tubes for transmitting messages have become extensively employed, and compressed air has become an important mode of transmitting power to a distance, the actual loss from friction of gases has come to be an object of special investigation. The experiments of M. Arson on the flow of air through cast iron pipes led him to infer that the coefficient of friction depended upon the velocity of flow in much the same way as the coefficient in the case of the flow of water. Still more closely is this the case in the results obtained from experiments on the transmission of power by air in the St. Gothard tunnel. The engineer, Mr. Stockalper, proved that the formula for the flow of water in pipes deduced by Mr. Darcy from the experiments I have already mentioned could be directly applied to calculate the flow of air, when the results are merely altered in the ratio of the density of air to that of water. Although Professor Unwin finds the value of the coefficient smaller than that given by Stockalper when the calculation is made to allow for the expansion of air, yet the similarity in the laws of friction for liquids and gases is very marked, as, for instance, in the considerable diminution in the coefficient of friction as the diameter of the pipe increases.

There is a remarkable and somewhat unexpected point of resemblance between liquids and gases in the fact that the friction is independent of fluid pressure. This result was arrived at from mathematical methods of reasoning by Professor Clerk Maxwell, in a paper read before the British Association in 1859. In the Bakerian lecture for 1866, on "The Internal Friction of Air and Other Gases," he published the results of elaborate experiments to test this theory. He found that the friction of air was the same for pressures between 30 inches and 0·5 inch, and, in fact, that the calculated and observed results agreed very well with each other.

In 1876, Mr. William Crookes, in experimenting with the revolving vane of a radiometer to test the friction of the residual gas, found that this law was nearly correct up to a certain point of tenuity of a gas, after which it entirely broke down. This led him to make a series of experiments upon the friction of a gas at high states of exhaustion, which gave results of the greatest interest in the study of the nature of gaseous friction. Maxwell's theory that the friction was independent of the density assumed that the mean length of path of the molecules between their collisions was very small, compared with the dimensions of the apparatus. Now, the mean length of path increases directly with the expansion, while the distance between the molecules only increases with the cube root of the expansion, and thus Mr. Crookes, who was able to work with pressures as low as 0·02 millionth of an atmosphere, obtained results which Maxwell's theory did not include. The method adopted was to observe the rate of decrease of the arc of vibration of a plate of mica suspended by a fine glass fiber twenty-six inches long in an exhausted glass bulb. Without attempting to give the numerical results obtained, the diagrams, Figs. 38, 39, 40, and 41, will enable



DIAGRAMS SHOWING EFFECT OF INTERNAL FRICTION IN GASES.

an idea of their nature to be obtained. Fig. 37 shows the damping action on the vibrations of the frictional resistance at 760 millimeters pressure, or one atmosphere,

* "Bulletin de la Societe Industrielle de Mulhouse," 1855.

† "Comptes Rendus," Nov. 17, 1884.

‡ Since these lectures were given I had occasion to carefully examine some of the results of Mr. Tower, and have called attention to a point which seems of practical interest. See *Engineering*, vol. xli., p. 438 *et seq.* 1886.

* Professor Thurston has suggested for the temperature of minimum friction at a pressure of 200 lb. per square inch the expression—

$$t = 15\sqrt[3]{V}$$

where V = the velocity in feet per minute. On either side of this point in the thermometric scale, he assumes the friction to vary either directly or inversely with the temperature, according as the temperature rises or falls.

and the quickly decreasing path of the mica plate in the successive swings is clearly seen. Fig. 39 is formed by joining the extremities of the length of successive arcs, and a logarithmic curve is thus obtained.

Now at 1 millimeter, or $\frac{1}{76}$ of an atmosphere, the result was almost exactly the same, and the logarithmic curve almost identical with Fig. 39. When, however, the exhaustion had proceeded much beyond this point, the form of the curve began to change, and by the time $\frac{1}{760000}$ of an atmosphere had been reached, the damping action was so enfeebled as to give the curve shown in Fig. 40, the corresponding logarithmic curve plotted as before from the successive arcs was no longer of the same form, but nearly a straight line, as shown in Fig. 41. These results not only throw important light on the nature of the internal friction of gases generally, but enable different characteristic properties of various gases to be examined. Thus the action of hydrogen was found to be entirely different to other gases; while on the one hand it has much less internal friction than other gases, it presents a very uniform resistance up to a high state of exhaustion, when it breaks down very suddenly.

Coming next to the friction of gases upon liquid surfaces, we have abundant evidence in nature of the reality and magnitude of this, not merely in the raising of ocean waves, but in cases, such as the flow of rivers, where the retardation of surface velocity is considerable with the wind up stream, while it is accelerated with the wind in the other direction. Thus it seems strange that Magnus, in 1850, should have denied the existence of an action of this kind as a result of certain experiments upon a jet of water. Viktor von Lang, however, found by causing the jet to flow through a tube, the sides of which it did not touch, that the friction was so great that the air seemed to adhere to the surface of the jet. An extremely interesting and important case of the friction of gases on liquids, mentioned by Clerk Maxwell in his book on "Heat," is the case of rain and mist. In this case a drop of water falling through air one thousand times rarer than itself, or about the density at the height of an ordinary cloud, would fall $\frac{6}{11}$ of an inch in a second if its diameter were $\frac{1}{1000}$ inch. If its diameter were $\frac{1}{10000}$ inch, it would fall only one-hundredth of its former rate, or half an inch a minute. A cloud of particles of this size would fall so slowly as to appear stationary. Thus it is on account of friction that clouds appear to remain suspended in the air, and that particles of dust, thrown up by volcanic agency or derived from meteoric sources, take so long a time to settle down.

Such are a few of the multitude of facts which might have been brought forward on the subject of fluid friction; but we must conclude this lecture with the consideration of a similar question to that at which we finally arrived in the first lecture, and endeavor to give a brief answer to the question, "What is the nature of fluid friction?"

In the first place, it must be remembered that there is an essential feature of resemblance between liquids and gases, which is, at the same time, the point of distinction between solids and fluids. In the case of solids, an alteration in form is only produced by stresses which exceed a certain value, and this is the case no matter how soft the solid may be. In the case of liquids and gases, a stress, however small, will cause a change of form, the extent of which depends upon the time of action of the stress. Thus we arrive at a definite mode of measuring the resistance to change of form of a fluid. Suppose one layer in the fluid to be in motion with a velocity, V, relatively to another imaginary layer at a distance, d, from it. Then if the intermediate fluid is in motion with a velocity proportional to its distance from either layer, there is a separation of the contiguous particles at a rate which increases in the proportion $\frac{V}{d}$. Now, the frictional resistance to this change is, in the case of fluids, measured by the area of the layers multiplied by this rate of increase, so that we may express the magnitude of the resistance thus—

$$F = \varkappa A \frac{V}{d}$$

when A = area of either layer,
$\varkappa$ = a suitable coefficient.

Thus for air the value of the coefficient has been found to be—

$$\varkappa = 0{\cdot}000,000,0256\ T,$$

where T = absolute temperature F.°

and the units are in pounds, feet, and seconds for water at 77° F.

$$\varkappa = 0{\cdot}000,00191$$

—(Piotrowski, quoted by Unwin).

The above coefficient is called the "coefficient of viscosity," and the property of the substance by which the resistance is caused is called its "viscosity."

I have hitherto carefully avoided the use of this word, which has here a definite meaning, although the highest authorities employ it as synonymous with fluid friction. Thus Clerk Maxwell says, "All fluids are imperfect, and exhibit the phenomenon of internal friction *or* viscosity, by which their motion, after being stirred about in a vessel, is gradually stopped, and the energy of motion converted into heat." ("Theory of Heat," p. 96.) Mr. William Crookes says, "By the viscosity *or* internal friction of a gas is meant the resistance which it offers to the gliding of one portion over another." ("Phil. Trans.," part ii, 1881, p. 388.) Rankine does not use the word viscosity, but says, "Although the particles of fluids have no transverse elasticity, it is certain they resist being made to slide over each other, and that there is a lateral communication of motion among them; that is, that there is a tendency of particles which move side by side in parallel lines to assume the same velocity. The laws of this lateral communication of motion *or* internal friction are not known exactly, but its effects are known thus far—that the energy due to the differences of velocity, which it causes to disappear, is replaced by heat in the proportion of one thermal unit, F, for every 772 foot pounds of energy, and that it causes the friction to take effect, not merely in retarding the film of fluid which is immediately in contact with the sides of the channel, but in retarding the whole stream." ("Applied Mechanics," p. 536.) Thus it is evident that the term internal fluid friction completely expresses the general phenomenon of resistance, and it would seem desirable to employ the word viscosity in a more limited sense, as the measurable resistance between parallel layers, as, for instance, Clerk Maxwell himself does in giving the following definition: "The viscosity of a substance is measured by the tangential force on the unit of area of either of two horizontal planes at the unit of distance apart, one of which is fixed, and the other moves with unit velocity." ("Theory of Heat," p. 298.) The term fluid friction could thus be used to denote all resistances, including those caused by eddying motion. This I understand to be the view held by Professor Unwin.

None of the foregoing passages, however, attempt to explain the ultimate cause of the resistance, and such explanation is only possible upon some hypothesis of the constitution of matter. The molecular theory enables definite reason to be given, which is set forth by Maxwell for the case of gases.

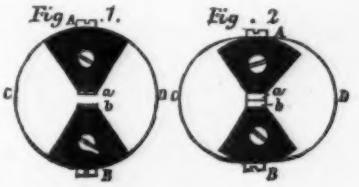
The molecules of gases are supposed to be in a state of continual agitation, and pass with great velocity from point to point. It is only when two gases chemically different are placed together that we can trace the passage of molecules in their diffusion, as it is called, of one gas into another. This diffusion is always going on in a single gas, only it is impossible to trace the progress of the molecules, because they are all identical. Maxwell explains the friction of gases on the molecular theory thus: "Particles having the mean velocity of translation belonging to one layer of the gas pass out of it into another layer having a different velocity of translation, and by striking against the particles of the second layer, exert upon it a tangential force which constitutes the internal friction of the gas. The whole friction between two portions of gas separated by a plane surface depends upon the total action between all the layers on one side of that surface upon all the layers on the other side." ("Phil. Mag.," vol. xix., p. 31.) Elsewhere ("Theory of Heat," p. 323), the same author says: "It appears, therefore, that diffusion, viscosity, and conductivity of heat in gases are related to each other in a very simple way, being the rate of equalization of three properties of the medium, the proportion of its ingredients, its velocity, and its temperature. The equalization is effected by the same agency in every case, viz., the agitation of the molecules."

The frictional resistance of liquids may be accounted for in a similar way to that of gases, though with them it must be confessed that there are considerable difficulties in the way of accepting an explanation of the above simple kind. Such explanation must take account of the property of adhesion in liquids—and possibly also in the case of gases.

Even the hypothetical explanation in the preceding paragraph does not completely answer the question, "What is the nature of friction?" We have seen in the statement of both Rankine and Maxwell that all the energy of motion is converted into heat, and that the internal friction is the agency by which this is done. The grandest generalization of modern times, viz., the mechanical theory of heat, rests upon this supposition, and the mechanical equivalent of heat was determined by Joule by means of paddles which were turned in a vessel of water by falling weights, the temperature of the water being ascertained for a given amount of work done by the weights. The fact that the molecules are made to take different velocities in passing from one layer to another of the moving gas does not of itself account for the rise of temperature due to friction, and we may suppose that the molecules themselves are capable of acquiring internal energy by virtue of a relative motion of their own parts, in addition to the increased energy of translation, and that the ultimate cause of frictional resistance is the absorption of energy by individual particles of the fluid. This, however, was exactly the point at which we arrived in the case of solids, and further than which we could not go; and we see that all substances, solids, liquids, and gases, have one common property in the capability of absorbing energy in the form of mechanical motion, which energy is converted into an equivalent amount of molecular motion of their internal parts. As in the case of solids, we have therefore, in the case of fluids, arrived at a point in the explanation of the nature of friction at which we must await further advance in our knowledge of the constitution of matter itself.

BUCKNILL'S CONTACT BREAKER.

WE annex illustrations of a very ingenious arrangement for making or breaking electrical contacts. It consists simply in mounting the contact points within



an elastic ring, by distorting which the points may be either separated or brought together, according to the normal condition of the contact. Fig. 1 shows a simple form of the arrangement, in which the points, a b, attached to blocks of vulcanite or other suitable insulating material, are mounted within a steel ring. By compressing this latter in the direction A B, or extending it in the direction C D, the points, a b, are brought in contact, and so maintained until the pressure is removed, when they separate instantly. It is evident that this arrangement is capable of numerous modifications, and that several pairs of contacts can be grouped within the same ring, or if desired they may be fixed so as to act outside. The system is adapted for a great variety of applications, and is now being employed by the British War Department and Admiralty, especially for use in land and sea mines. The contacts are manufactured by Elliott Brothers, London.

THE MOUNTING OF AN ELEPHANT.

THE common practice in mounting large mammals is to first make the legs, and, having fastened them securely to a backbone of plank, to pack the remainder of the body with loose filling. While this does well enough for long-haired animals, whose muscles are concealed, for those that are scantily clad some other methods must be adopted in order to reproduce correctly form and features.

To build up an animal that will be lank and flabby is the height of taxidermic art, and a brief description of the manner in which the elephant Mungo was mounted at the National Museum will show the methods by which such results may be obtained.

Mungo was an African elephant about six years old, belonging to Forepaugh's menagerie, that thoughtfully selected Washington as the place of his demise. Mr. William T. Hornaday, the distinguished taxidermist, saw in this event an opportunity of putting the new principles of mounting into practice.

The first step in the process of mounting was to take a series of careful measurements of the body, showing its length, height, and girth at various points, and the dimensions of the limbs and the trunk. These were supplemented by sundry drawings and by plaster casts of the head and of the limbs of one side. The more care was necessary in this, owing to the fact that the entire skeleton was to be mounted separately, and thus no guide left to the position of the joints. This done, the skin was removed, and transferred to a bath of salt and alum, there to lie until its false body should be ready.

The backbone of this false body (Fig. 1) consisted of

FIG. 1.—SKELETON OF WOOD AND IRON, THE BODY WRAPPED WITH ROPES OF TOW.

a broad two inch plank, the upper edge of which was carefully cut into an exact copy of that dorsal outline which is so characteristic of the African elephant. To this the legs were attached by heavy angle irons, the iron that formed the axis of the leg running through a hole in the free arm of the L. The legs themselves were formed of excelsior solidly wound around roughly hewn wooden bones.

The accuracy of the work was proved by frequent reference to the measures taken from the dead animal, due allowance being made for the fact that the finished piece would be somewhat larger than its supporting framework. The broad overhanging pelvis was next added, and then the skull, with its massive jaw, was built on, the more salient portions being carved with care from the plaster model, while those buried deeply in the flesh were more roughly copied.

The long ribs of the original were represented by bands of iron wrapped in tow, fastened above to the plank backbone, and below to a second plank shaped

FIG. 2.—SKULL, BACKBONE, AND SCAPULA OF WOOD, AND WRAPPING OF EXCELSIOR.

to the outline of the under side. A neck of laths, covered with excelsior, joined the head to the body. Wooden shoulder-blades were now put in place, the tail and trunk added, and then, following the diagrams and accompanying measurements, the vacancies existing between the upper parts of the legs and adjacent portions of the body were carefully filled out (Fig. 2). Like Mother Rigby's Feathertop, the elephant at this

stage stood forth a creature of wood and tow, only waiting for the final metamorphosis which should fill the blank wooden orbits with twinkling eyes and endow the entire framework with the semblance of life. The thick, stiff skin was now removed from the bath, and carefully thinned down until it had lost half its substance, and become, for an elephant, soft and pliable.

As a careful tailor tries on a partially completed coat to assure himself that the finished garment will be a success, so the skin of Mungo was hung upon the manikin (Fig. 3). The trial proving satisfactory, the skin was poisoned with arsenical soap, and all was ready for the last act. The skin having been replaced and se-

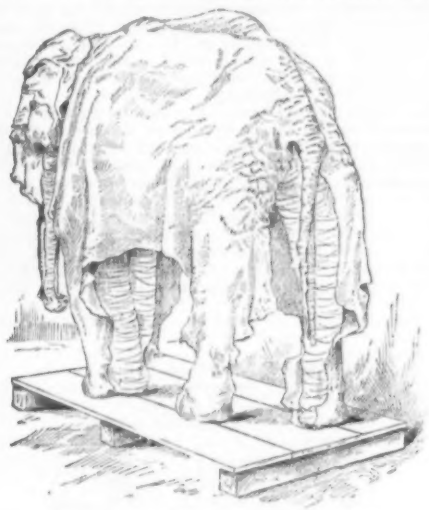


FIG. 3.—TRYING ON THE SKIN.

cured along the back, first one side, then the other, was covered with a thin coating of clay mixed with chopped tow, and the body section sewed up. One by one the legs, trunk, and tail were similarly treated, the skin being covered each night with wet cloths to preserve it moist and flexible throughout. The sewing having been finished, the wrinkles indicated in the skin were followed over with a pointed modeling tool, thus impressing them deeply in the moist clay, while the deepest wrinkles or thick folds of the trunk, elbows, and flanks were secured by wires or twine to hold them in place until dry. Great pains were taken in inserting the eyes—made from a color sketch of the originals—and marking in their surrounding lines, on which depended the expression of the face. After thoroughly drying, all seams were filled with *papier-mache*, while a slight but careful use of color restored the skin to its original aspect (Fig. 4).

Thus was Mungo reconstructed, and thus did Mr. Hornaday successfully solve the problem of so mounting an elephant that his hide should appear loose and wrinkled, instead of, as is too often the case, smooth and swollen. Mungo was exhibited at the Washington meeting of the Society of American Taxidermists, and received the special medal "for the best piece in the entire exhibition. This elephant may be said to represent the beginning of the new and better class of taxidermy at the national museum; and although four years ago he stood, as regards quality, almost isolated, he is to-day surrounded by so many pieces of equal merit that we may look hopefully forward to the time when the mounted mammals of the national museum shall be unsurpassed.—*Frederic A. Lucas, in Science.*

The foregoing brings to mind the career and tragic death of Jumbo, one of the noblest elephants, in respect to size, ever held in captivity.

Twenty-six years ago the London Zoological Society procured from Paris a baby elephant, which, in process of time, grew to be a creature of immense size, and for several years the beast had been used in the Zoological Gardens to carry about the loads of children and visitors that were always clamoring for an elephant ride. The great animal, named Jumbo, as he increased in size and years developed also some bad traits of character, becoming so vicious that the directors finally decided, in 1882, to get rid of him. So Mr. P. T. Barnum, of this country, became the purchaser for $10,000. But to get him on shipboard and carry him to New York was no slight job. There was great lamentation among the juvenile fraternity of London when the departure of Jumbo was announced.

Jumbo was a universal favorite, and as gentle with children as the best trained poodle dog, taking the proffered biscuit or lump of sugar with an almost incredible delicacy of touch, so that the most nervous child, having once overcome his alarm, never hesitated to hand a morsel to the waving trunk a second time. Jumbo, however, when in his house displayed a very uncertain temper at times, and this, as stated, induced the Society to part with him. The risks of an outbreak on the part of so huge and powerful an animal in the much frequented gardens of the Society is one which should be not lightly run. In Mr. Barnum's establishment, where twenty elephants are kept, an animal can be withdrawn and placed in seclusion, for which there was no adequate provision in the Zoological Gardens.

Mr. Barnum agreed to take all risks of removal. Mr. Barnum, however, had reckoned without asking Jumbo, who declined to consent to the arrangement, and resisted all the blandishments of Scott, his favorite keeper, who had been told off to accompany him throughout the voyage. Saturday had been fixed for his removal to the docks, and chains having been passed round his legs and body, Jumbo soon felt that something was wrong, and gave vent to loud trumpetings of dismay and anger, while vigorously trying to free himself. The other elephants, hearing his cries, joined in chorus, and the female, Alice, or, as she is called, Jumbo's "little wife," was almost beside herself with anxiety. By and by, however, Jumbo calmed down, and Scott and the well known American expert, Mr. William Newman, otherwise known as "Elephant Bill," who had been sent to superintend the removal, attempted to induce him to enter the box on wheels, in which he was to be conveyed to the dock. This, however, he absolutely refused to do, and finally he was left quiet for the night. Next day it was decided to attempt to lead him through the streets. Jumbo walked calmly enough to the entrance, but then, feeling a different soil under his feet, became once more alarmed, and refused to proceed further. "Then ensued," states a writer in the *Daily Telegraph*, "one of the most pathetic scenes in which a dumb animal was ever the chief actor. The poor brute moaned sadly, and appealed in all but human words to his keeper, embracing the man with his trunk, and actually kneeling before him. Jumbo's cries were soon heard by his 'little wife,' who quickly responded with loud trumpetings, at the sound of which Jumbo became frantic, and flung himself down on his side." It now became evident that he could not be induced to march through the streets, and so Scott, to his great joy, led him back to his house, where Alice received him with gambols and great glee. Jumbo was left in peace for a fortnight, a large box on wheels being placed at the entrance of his house, so that he was compelled to pass through it, and in this manner became accustomed to its appearance.

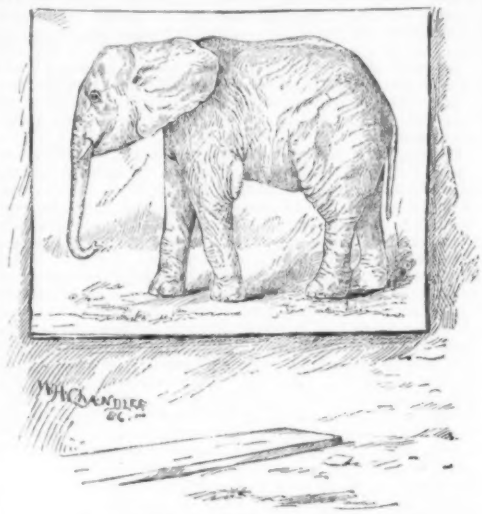
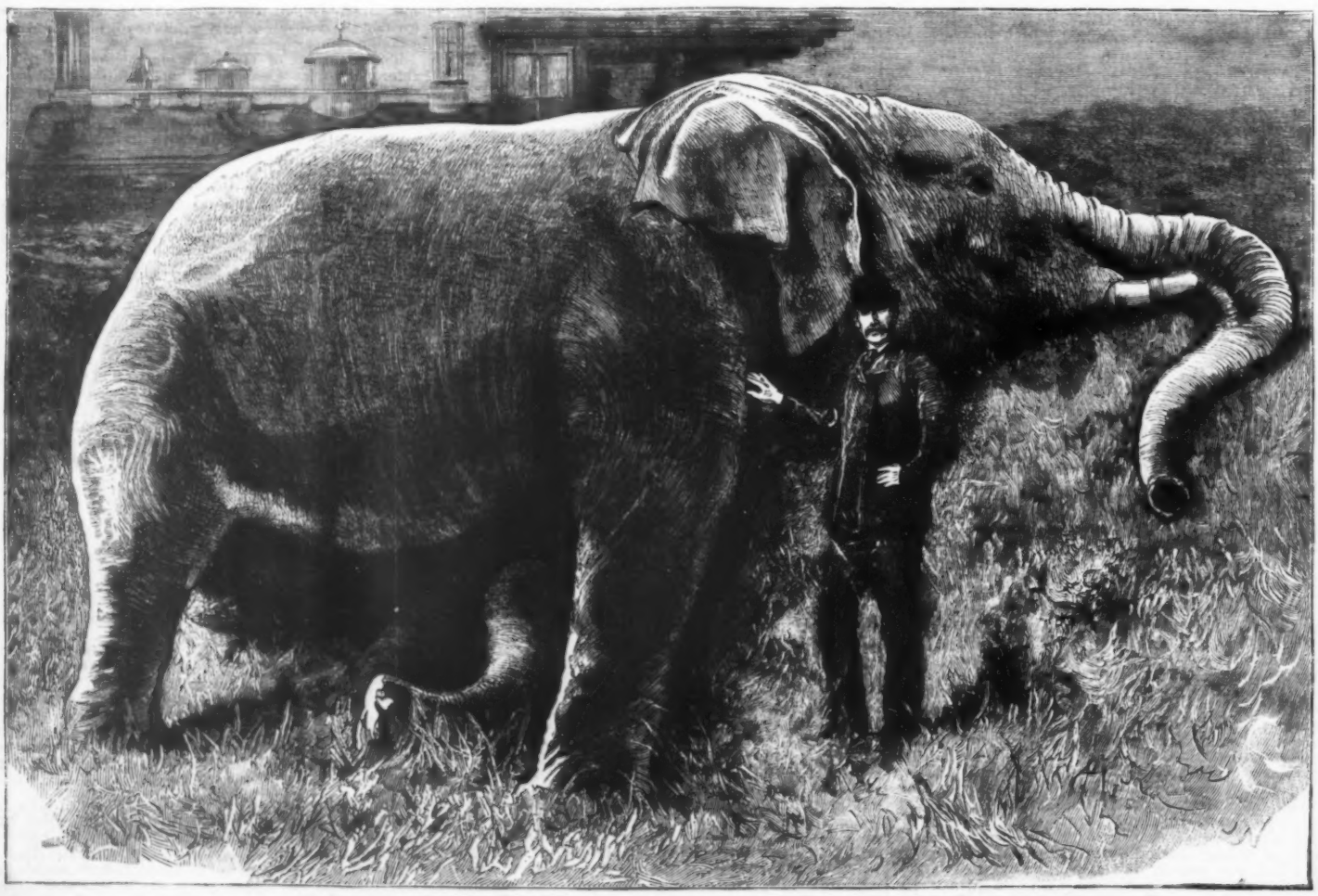


FIG. 4.—THE ELEPHANT FINISHED.

Jumbo was twenty-one years old when he left London, having come to the gardens from Paris at the age of five, in exchange for some other animals, and then was about the size of a Shetland pony. He was eleven feet high, and the largest elephant in Europe, and, according to the testimony of African travelers, a giant among African elephants, who rarely exceed the height of ten feet. Mr. Bartlett attributes this rapid and remarkable life to the good feeding, the careful housing, and constant grooming and attention which were accorded to him during his stay in the gardens.

After long delay, Jumbo was successfully placed in the box which had been prepared for his transportation. At about 9:20 the doors of the stable were opened, and Jumbo was led out for the last time into his paddock by Scott, the elephant being followed by Newman and the assistants. When Jumbo reached the entrance to the box, he, for an instant, hesitated, and then, with his chains clanking behind him as he walked, he quietly stepped into it, and would have gone through into the grounds but for the well-known "Whoa, Jumbo!" from Scott. As the elephant stopped the chains attached to his forelegs were slipped through holes at the side of the box, and round



JUMBO, THE GREAT AFRICAN ELEPHANT, KILLED BY A LOCOMOTIVE.

strong staples bound with iron fitted to one of the uprights. Then, inch by inch, these were drawn up tight, so that Jumbo stood with his fore feet about three feet apart. Now came the task of hobbling the hind feet, and fixing them in the same way as the others. As fast as an attempt was made to pass the loop round his one foot, he contrived to step upon it with the other, and evade it; now and again, as other means failed him, going down upon his knees and stretching himself to the utmost. Time after time did the attempt to snare him fail, but after about half an hour of patient work the hobble was fixed on the leg, and the next instant passed around the staple. This was accomplished about 10:30, or two hours and a half from the commencement of the business. During the afternoon a number of men were digging away the ground in front of the box, and sloping it upward to form an inclined plane. When this was accomplished, and planks had been laid down, six strong horses were harnessed to the huge car, and the task of drawing it up to level ground commenced. . . . Night came on, and the work proceeded slow by lamp light. The car had traveled one hundred yards in five hours. The night was clear and starlit, with a crescent moon in the sky and a chill wind blowing. Jumbo was protected from the keen air by tarpaulins stretched across the bars of the two ends of his box.

At 10 o'clock the horses were taken out and led away. At about 10:15 the elephant began moving about uneasily in his box, put his trunk out, played with the straw in front, fished up one of the nosebags of the horses, and afterward threw it out on to the head of an attendant. Then, less good humoredly, with short, quick jerks of his hind legs, he made his chains rattle again. Next he began to jog rapidly backward and forward, butting as he came forward against the front bars of his cage, and making the ponderous box move backward and forward with his weight. Great clouds of steam burst from his trunk into the cold night air. As he felt his strength tell he appeared to redouble his exertions. The box rattled, and its frame quivered at every blow. It seemed merely a question of time to break it. If he had continued these tactics long enough nothing could resist them. One of the attendants spoke to him, and quieted him for a moment, but Jumbo began again. At length Scott, who had been taking supper, returned. The elephant was calmed at once, and shortly after had food and water. It may be stated that Jumbo snapped like tow a four inch rope which was used in fettering him.

At 11:15 Jumbo began to dance again, snorted, knelt down, worried his chains with his trunk, shook his box heavily, and would certainly have broken it if he had persisted; but at the command of his keeper he became quiet. At 12:15 the horses were put to; at 12:50 they tried to start, with ten horses harnessed two abreast, but the wheels had sunk so deep in the earth that it was impossible to move. Jacks were now employed to lift them, and at 1 o'clock a start was fairly made. At 1:25 the car was safely out of the gardens, and rattling off on the road to the docks.

The docks were reached at about 5:30 in the morning, the distance being about five miles. By this time it was almost daylight, and although it was now too late to think of utilizing the early morning tide for enabling the barge to leave the dock with Jumbo on board, it was decided to hoist the box on the lighter at once. Stout hempen ropes were passed round the structure in which Jumbo was confined, and both the elephant and his temporary prison van, weighing together between ten and eleven tons, were hoisted into the air and gradually lowered into the barge Clarence, which floated alongside the dock.

From the barge the caged elephant was readily transferred to the steamer Monarch, and in the course of twelve days safely landed on the wharf in New York. Thence he was conducted to the elephant home of Mr. Barnum, at Bridgeport, Conn., and after a recuperation in that comfortable place, Jumbo made his appearance as a regular member of "Barnum's greatest show on earth." This magnificent animal proved a great attraction to Mr. Barnum's menagerie, and soon repaid the indefatigable manager for all the money and time he had spent in bringing him to the New World. For several years wherever the show went Jumbo was the conspicuous star.

The immense animal had just been exhibited at St. Thomas, Ontario, on the 15th of September, 1885, and was being led along the railway track with Tom Thumb, the baby elephant, to be loaded into their respective cars, when a heavy freight train, running at the rate of forty miles an hour, bore down upon them. As soon as Jumbo saw the train, he made a rush for Tom Thumb, and grasping him in his trunk, threw him away across the tracks as easily as if he had been a kitten. Jumbo then tried to save himself, but it was too late, and he was crushed to death between the engine and the cars on the siding. The poor beast's devotion was unfortunately wasted, for Tom Thumb's leg was broken, and he had to be shot the following day. The engine and several cars of the colliding train were thrown from the track.

Our engraving shows the appearance of Jumbo after his death, as he lay against the side of the railway embankment.

The remains of Jumbo continue as a double attraction in Barnum's show. His bones have been finely mounted, and his skeleton forms a remarkable object to look upon. His skin has also been stuffed in the best manner, by the process here illustrated. Skin and skeleton are thus made to contribute to the curiosity of the public and, in some degree, to the advantage of science.

NEBULÆ AND PLANETS.

By H. GOUPIL.

THE stars composing the same system are submitted to mechanical laws which secure the equilibrium of their various motions, and, in a large measure, protect their existence against the destruction that might result from disturbing causes. The forces that concur in such equilibrium are attraction on the one hand and inertia on the other.

Attraction may be considered as an active force. It is, in a manner, the life of matter, comparable with muscular energy, which, in an organized being, is controlled by its will. Inertia, a passive force, seems to be designed for balancing the other, and to be an essentially material property. It takes the place of a force, but is not one in the proper sense of the word.

In a revolving system inertia manifests itself through centrifugal force. If, in such a system, we consider any congeries whatever, a nebulous agglomeration, for example, in a cosmical state, centrifugal force, which tends to make an agglomeration of this nature fly from the center, acts upon it precisely as if it were unique in space. The rest of the mass has no influence upon the said force. The centrifugal force, then, that acts upon this particular nebulous particle is exclusively connected with two factors, viz., velocity and the distance to the axis of revolution.

The same is not the case with attractive force. That which acts upon any molecule whatever will depend upon the mass as a whole and the distribution of the same in the space existing around the nebulous particle.

It results that the distribution of the centrifugal and attractive forces that act upon the different parts of the cosmical mass is very variable, according to the distances to the axes of revolution; and that the equilibrium in the various parts of the whole will be much altered if, by reason of its very molecular constitution, the distribution of the materials that form the mass is itself variable.

It is easy to see what foundation there is for the theory of the formation of stars through successive rings detaching themselves from a cosmical mass. Let us, in the first place, imagine two cosmical masses having convergent directions in space. Let us remark that there already exists an infinite number of chances to one that the direction will not be exactly upon the line of the centers, and that the two masses will not be equal in bulk, density, nature and velocity. According to the laws of attraction, they will approach each other with accelerated velocities; and if they were first spherical, they will become elongated, because the parts in conjunction will attract each other more strongly than those that are in opposition. They will therefore deviate from their primordial directions in describing two curves, and, at the moment of contact, will have the appearance shown in Fig. 1.

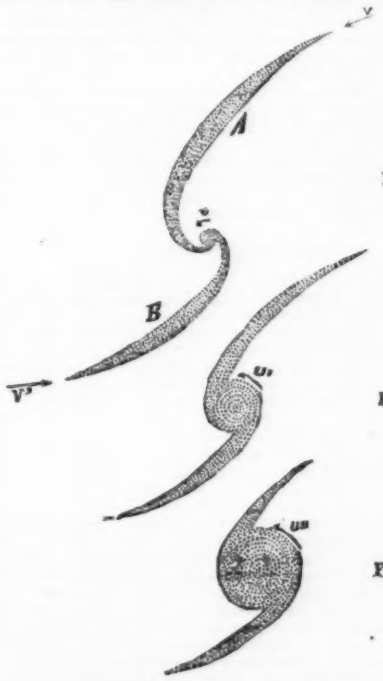


Fig. 1.
Fig. 2.
Fig. 3.

The velocities required, then, will, at the moment of union of the two coalescing masses, constitute the different velocities of revolution at each point of the radius of the vortex that is going to form. In the beginning, the parts tend to form a spherical nucleus; but the mass increases in bulk and intensity in measure as material arrives, and the attraction is therefore increasing so that the newly arrived parts have velocities greater than the first. As these velocities are sensibly tangential, they give the new concentric zones rotary velocities greater than those of the center; and the whole becomes lenticular, and vaster and vaster.

Finally, the last parts unite in directions less tangential, by reason of the increase of the sphere's diameter; their components in a tangential direction give less gyrating velocities; and it follows that the parts that have the greatest linear velocity of gyration are the median ones (Figs. 2, 3, and 4).

Figure 4.

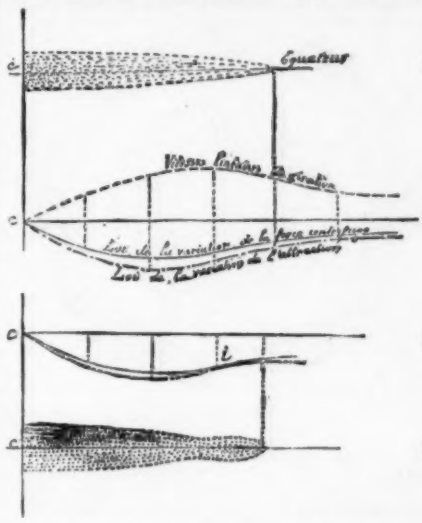
As the nebula thus formed is composed of two masses that are of unlike nature, density, etc., it is not homogeneous, either as a whole or in the same concentric zone.

In time, such homogeneousness will occur; but the slight density of the whole, its large dimensions, and the slight power of the attractive forces, defer the formation to so far off a date that the disturbances created by this want of homogeneousness have time to disunite the various substances of the nebula, and to convert it into a planetary system.

It is plain that if, at all parts of the mass, the attractive force were exceeded by the centrifugal, the nebula would form a ring, which would increase until an equilibrium was established. The lenticular form, on the contrary, satisfies the opposite condition.

It may occur that the last portions that are proceeding toward the vortex shall escape it through the great velocity that they have acquired; in which case, according to the laws of Kepler, they will pursue their route in space with very elongated orbits, and may thereafter form periodical comets, or never return again.

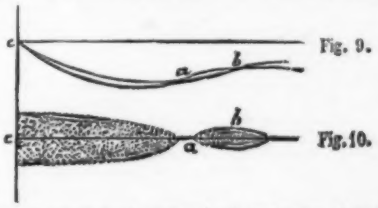
The lenticular nebula (Fig. 5) will be governed by



centrifugal and attractive forces, whose laws (Fig. 6) will be such that the former will at all points be superior to the latter. But such laws will become modified in different ways. By very reason of the fact that, at every place, attraction exceeds the centrifugal force, the concentration takes place through the slow compression of the gaseous fluids and the equally slow precipitation of the cosmic dust; consequently, an increase in the rotary velocity and centrifugal force.

The attraction further increases by reason of the reduction of the radii; so that if, during such transformation, the attractive force is constantly surpassing the centrifugal at every place, the nebula will finally resolve itself into a fixed star. But, as the two laws are not connected by one principle, other combinations are possible. In the first place, a point of contact may form (Fig. 7), and, later on, a crossing and recrossing (Fig. 9).

At *a* and *b*, in Fig. 9, the two contrary forces are



equal, and every part comprised between the circumference, *a*, and the center will continue to concentrate, since attraction is surpassing the centrifugal force there. But, for a contrary reason, the parts between *a* and *b* will become more distant from the center, while those situated beyond the circumference will approach the center. Therefore there will be a concentration at the center and upon the circle *b*, and then upon the circle *a*; hence the formation of a ring (Fig. 10). This ring, when once separated from the mass, will be free, and will preserve its rotary velocity independently of the transformations that it is to undergo.

The rest of the nebula will continue to concentrate, and the slow modifications in the attractive and centrifugal forces will soon lead to new inequalities and expurgations favorable to the detachment of a new ring, until the concentration becomes so great as to no longer lead to such a result. The central core will become more and more spherical, and it is easy to see that, since the cause that creates the rings keeps on getting weaker, the latter will become of less and less bulk, as will also the stars that result therefrom.

Again, it will be seen that while the first rings are larger in bulk, they may be less in ponderable mass than the secondary ones, by reason of the fact that they have been formed by the nebula's zones of least density, and at an epoch when all the parts were still very near their minimum of density. It seems logical, then, that the secondary rings will furnish stars of the greatest mass (Jupiter).

We now come to the conversion of the ring into a sphere.

We may readily ascribe the reason of the breakage of the rings to their want of homogeneousness, the source of which we have already explained.

Say we have a ring (Figs. 11 and 13) whose mean circle, *b* (Fig. 10), has a certain velocity. This ring will continue to concentrate itself upon the circle, since it is in a state of equilibrium with respect to the rest of the nebula. There remain, as transforming forces, the attractive action upon each other of the same parts of the ring, since the attractive action of one point, with respect to that of another one diametrically opposite, is imperceptible, seeing the enormous distance that separates them. It is not the same with the parts which are approximate, and which act upon each other throughout the entire circumference of the ring. The latter, then, would be indefinitely preserved were it homogeneous, since these actions would balance each other at every spot.

But, having admitted (Figs. 11 and 13) that a portion, A, of the ring (Fig. 11) is denser than another portion, B, a greater concentration will occur at A than at B, and, like a drop of oil that spreads out in a circle, the mass will tend likewise to flow before the ring. It will go along the latter, become rarefied at B and accumulate at A, and the ring will soon take on the form of a crescent, and afterward become concentrated at one spot,

where it will form a small nebula, in every respect like the mother nebula, while at the same time preserving its rotary velocity around the latter.

Let us now see what the direction of such a rotation will be.

In passing from B toward A, and that, too, during the general rotation, the various particles acquire accelerated velocities, which on the side C are additives of the velocity, V, of rotation, and on the side D are reductives of the same. There will form, then, two tails, one of which—that on the side C—will increase its centrifugal force, and the other—that on the side D—will decrease it. One of these tails will be within the mean circle, and the other extend to it, and for these reasons the little nebula will be found in the condition shown in Fig. 3.

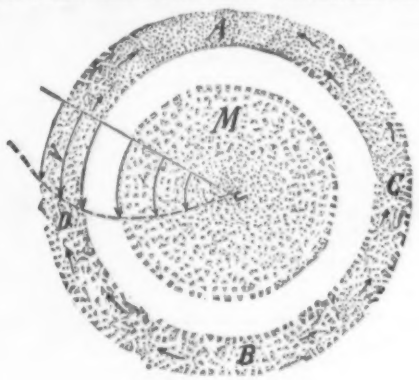


Figure 11.

When these tails have became disengaged, they will tend to communicate their live power to the mass, one of them contrary to the other, that is to say, to bring about a rotary motion in a direct sense. But, if it happens (Fig. 11) that the linear velocity is greater

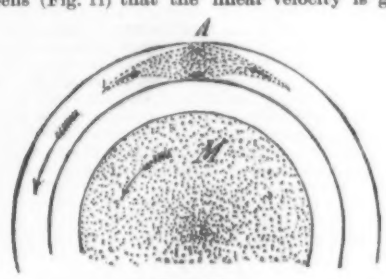


Figure 12.

near the internal edge of the ring than at the outer, the effect will be to produce a retrograde motion, while, if the contrary is the case, the motion will be direct. In the first case it will therefore depend upon the relative sizes of the masses, A, and of the tails, whether the motion will be retrograde or direct.

As in Neptune and Saturn the rings are very large, the fall of the tails upon the place of concentration has occurred without great deviation in the mean circle, the great radius of the latter, and the feebleness of the centrifugal and attractive forces of the mother nebula;

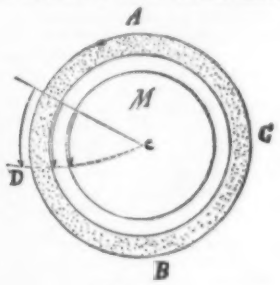


Figure 13.

and, moreover, as these rings are inside, it has been possible for a relatively large excess of internal velocity to exist over the external. Then the tails have been insufficient to prevent the retrograde motion resulting from the latter fact.

The contrary is the case with smaller rings (Fig. 14). Finally, we shall remark that an asymptotic law, decreasing on moving away from the center in the angular velocities of the different parts of the nebula, may give linear velocities that first increase up to a certain distance, and then decrease; in which case, all the rings

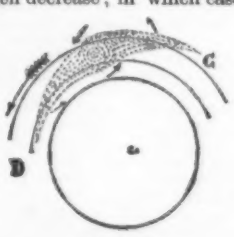


Figure 14.

that become detached beyond the place of maximum velocity will give stars that have a retrograde motion, and all others beyond will give stars that have a direct motion. Such factors, then, as mass, extent, bulk, and distribution of matter, play an important part, and the effects may be various. They are, so to speak, of chance origin, although governed afterward by fixed laws. It is due to chance that a hen has 5, 8, or 12 eggs to sit upon, but, whenever the latter are hatched, fixed laws preside over the development of the chickens.

If the rings are very homogeneous, they will not break; but if some resistance reduces their rotary velocity, they will close up upon their center (Saturn).

It is obvious that, under the velocity of gyration, the nebula derived from a ring will flatten, and perform the evolutions already pointed out. It will detach other rings, which will form planets. These phenomena occur so much the more rapidly in proportion as the mass is smaller; consequently, the moon, although formed after the earth, may be older than the latter, Jupiter, detached before the earth, may be younger than she, and, finally, all the planets are older than the sun, which is nevertheless their father.

A ring having been detached, the rest of the nebula, by reason of the laws of inertia, may move its axis forward in space, and the same is the case with secondary nebulæ, when they have given off the secondary rings. The result will be various obliquities in the planes of the rings and in those of the orbits, and also obliquities of the planets, which are themselves formed upon their own orbits.

Finally, every solar nebula may have been the transformation of a vast ring detached from a still larger nebula. Upon becoming a nebula, a broken ring increases the mass at the place of concentration. A slight modification takes place in the attraction, and hence an approach toward the primitive center until the increase in velocity resulting therefrom leads to a contrary excess, from whence oscillation. Admitting that the ring has first described a perfect circle, the star formed will, according to the laws of Kepler, follow an elliptical orbit.

Different places of concentration may exist upon the same ring, and, in this case, if two of such places are diametrically opposite, two secondary nebulæ will follow the same ordit; but, however slight be the difference in velocities, these nebulæ will certainly unite, and if the stars are then in process of formation, there will be a shock. Or, if the two masses get to a sufficient distance from each other, a two-star system will be constituted, one star gravitating around the other, unless they coalesce, as shown in Fig. 1.

The system of the nebulæ, then, seems to satisfy all the combinations possible; and if the theories according to which the formation of the planets and their satellites has been deduced be true, we should find in these great phenomena, as we find in smaller ones, and as in the moral evolutions of peoples, some fundamental principles, which through the intervention of slight causes of attraction, a little disorder, and a few ferments of discord, lead to evolutions and transformations whose causes escape our observation, but which must co-operate, whatever they be, to one final end, to an unknown ideal.—*Chronique Industrielle.*

THUNDERSTORMS.

AT a recent meeting of the Physical Society, Berlin, Prof. Bornstein communicated the results of his investigations into the thunderstorms of July, 1884. The days from July 13 to 17 were very prolific in thunderstorms, and respecting them the speaker had collected and elaborated observations from more than 200 stations in Germany. For 24 separate thunderstorms, drawings were made of the "isobronts," isobars, and isothermals, from which it appeared that a fall in the barometer always preceded the outburst of the storm; that with the occurrence of the sinking of the barometer the atmospheric pressure rose very steeply and then relapsed gradually to its former level; and that the temperature, which was very high before the storm, declined rapidly with the outbreak of the storm. Local observations had formerly led to the same result. The "isobronts," or the lines uniting the places where the first peal of thunder was simultaneously heard, had in general a north-south direction. The "isobronts" made the passage from west to east with an average swiftness of from 38 to 39 kilometers an hour. The "isobronts" were attracted by the mountains, so that the part in whose west-east direction a mountain was situated approached it sooner, and, after the passage of the "isobronts," delayed there longer than did the remaining part. Rivers retarded the progress of thunderstorms, and small thunderstorms often terminated at large rivers without crossing them. This relation of thunderstorms to mountains and rivers might be explained on the assumption that the storms were caused by ascending air currents. When such an ascending air current approached a mountain, then the mountain hindered the horizontal air from flowing in at the anterior side of the ascending current. The air flowing in at the posterior side, on the other hand, thereby obtained the preponderance, and urged the phenomenon with all the greater force to the mountain. The reverse occurred after the thunderstorm had surmounted the mountain. The horizontal currents in front then obtained the preponderance, and delayed the progress of the storm. The influence of the rivers found its explanation in the fact that the air above the water was considerably cooler than the air above the land, whereby a descending air current was continuously maintained, operating in opposition to the ascending current of the thunderstorm, to the possible degree even of annulling it. The speaker had been able artificially to produce an imitation of all these processes by causing, in accordance with the directions of Dr. Vettin, visible currents to ascend in a glass box filled with tobacco smoke, by means of local depressions of temperature, by setting these currents in constant motion, and making them strike against obstructions (corresponding with the mountains), as also on descending currents, which were likewise artificially created. In the discussion which followed the above address, Dr. Vettin laid stress on the fact that precisely at the moment when the barometer mounted steeply from its lowest position the thunder followed the lightning most rapidly, and discussed how, in accordance with his conception of the nature of thunderstorms, by the curving round of the ascending air current, a whirling movement round a horizontal axis came into shape, whereby, as determined by its situation and its extent, were produced thunderstorms, sleet, and hail.

Prof. Von Helmholtz described the formation of a thunderstorm, observed by him in Rigi-Kaltbad. From a free point of prospect, allowing a survey of the plain as far as the Jura, he observed how the lower warm and moist layer of air was distinguished by a sharp horizontal boundary of somewhat long strips of cloud from the upper dry and cooler air. The cloud masses resembling the stripe-shaped cirri diffused themselves and formed a coherent level boundary layer between the two air masses. He next noticed, at different spots, balls of cloud arise above the boundary layer, evidently as the effects of ascending air currents. The different cloud heaps then rose higher and grew into larger cloud masses, within which different electric sparks leaped from one spot to another. It was only subsequently that he saw the lightning fly downward to the earth. At last a heavy rain rendered the lower air mass, bounded by the horizontal cloud basis occupying a position nearly at a level with the height of the standpoint, which had hitherto been clear, opaque. The phenomenon had developed itself under weather in which the wind was at rest, and could be followed very precisely into its details.—*Nature.*

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